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## ORIGINAL ARTICLES.

### I.—On '*Pyrgoma cretacea*,' a Cirripede, from the Upper Chalk of Norwich and Margate.

By Henry Woodward, LL.D., F.R.S., V.P.Z.S., F.G.S.

(PLATE VIII, Figs. 1–5.)

In the year 1865 I noticed the occurrence of what appeared to be a sessile Cirripede from the Upper Chalk of Norwich, and referred it to Leach's genus *Pyrgoma*. For this unique example the name of *Pyrgoma cretacea* was then proposed,[1] and afterwards, in 1868, it was more fully described and figured by me in the Geological Magazine.[2] I also pointed out that Charles Darwin, in his Monograph on the Fossil Cirrepedia,[3] had described a fossil form belonging to this genus under the name of *Pyrgoma anglicum*, from the Coralline Crag of Ramsholt, Suffolk, a species found living off the south coast of England and of Ireland, Sicily, Madeira, Cape de Verde Islands, etc.; while Michelotti had named, but not described, a species (*Pyrgoma undata*) from the North Italian Tertiary strata.

The only other form of sessile Cirripede known, which extends back in time to the Chalk formation, is the genus *Verruca*, which M. Bosquet of Maestricht first described in 1853 from the Chalk of Limbourg under the name of *Verruca prisca*.[4] This species was likewise discovered by J. de C. Sowerby in the Upper Chalk of Norwich, and described under the same name by Charles Darwin.[5] Like the genus *Pyrgoma*, *Verruca* occurs fossil (*Verruca Strömia*) in the Glacial beds of Scotland, the Red and Coralline Crag of Suffolk, and recent on the shores of Great Britain and Ireland, etc.

[1] Brit. Assoc. Birmingham (1865), Reports, p. 321.
[2] Geol. Mag., Dec. I, Vol. V (1868), pp. 258–9, Pl. XIV, Figs. 1, 2.
[3] "The Fossil Balanidæ and Verrucidæ": Pal. Soc., 1854, p. 36, tab. ii, fig. 7.
[4] J. Bosquet: "Mon. Crustacés foss. terr. Crét. Duché de Limbourg," p. 14, figs. 1–7. Darwin makes a distinct family for this genus—the Verrucidæ.
[5] Mon. Pal. Soc., 1854, p. 43, tab. ii, fig. 10.

Darwin, in describing the genus *Pyrgoma*,[1] says:—"The shell consists of a single piece, generally without suture, even on the internal surface; and this is the case, at least, in *P. anglicum*, in extremely young colourless examples: nevertheless, in some specimens of this very species, and of *P. conjugatum*, there were traces of two, *but only two*, sutures on the sheath, one on each side towards its carinal end. The shell is often much depressed or actually flat; in *P. anglicum*, however, the shell is steeply conical. The outline is rather oval. The surface is furnished with more or less prominent ridges, radiating from the orifice, which is oval and small." (See Pl. VIII, Fig. 5.) "The shell," he adds, "is unusually thick."

"The basis, in all the species, is more or less regularly cup-formed or sub-cylindrical. In *P. grande* it penetrates the coral (on which it is fixed) to a surprising depth; but this is not the case with *P. anglicum*, in which the basis is generally exserted, as it is in a slight degree in *P. grande*."

Of the opercular valves in the Chalk species, so important and essential in the study of any of the Cirripedia, we still remain in ignorance. I should not, therefore, have ventured to reopen the previous description of the so-called '*Pyrgoma cretacea*,' had it not happened that a new and important light has been thrown upon it, quite unexpectedly, through the discovery in the Chalk of Thanet of a second specimen by my friend Dr. Arthur Rowe, M.S., M.R.C.S., F.G.S., of Margate. This gentleman's admirable researches on the zones of the English Chalk have greatly added to our knowledge of its detailed stratigraphy, whilst, by the application of the dental engine for the development of minute and delicate organisms preserved in the Chalk, he has made geologists acquainted with a host of beautiful and novel organisms, among which the present addition to our knowledge of the form hitherto known as '*Pyrgoma cretacea*' is not, as I hope to be able to show in the sequel, the least interesting and instructive contribution.

Towards the close of last year, Dr. Rowe brought me the specimen which is the subject of the present communication, and which is figured (enlarged three times) on Pl. VIII, Fig. 4*a*. The original specimen obtained from the Chalk of Norwich, and described by me in 1865 (see Pl. VIII, Fig. 3), consists of nearly half the circumference of the conical walls of the shell, the opercular valves and the basis being absent.

I attributed the absence in the Norwich specimen of the characteristic cup-formed basis, usually seen in *Pyrgoma anglicum* and other species of that genus, to the readiness with which the conical walls of the shell separate from the basis, owing to a cleft covered by a membrane which may be observed all round between the lower edge of the shell and the basis in many of the species. In referring this Cretaceous Balanid to *Pyrgoma*, I was influenced by the following considerations, namely: (1) the steeply conical form of the shell-wall (see Pl. VIII, Fig. 3); (2) the rounded approximate

[1] A Monograph of the Subclass Cirripedia, etc.: The Balanidæ and Verrucidæ, p. 355. Ray Society, 1854.

radiating ribs which ornament the surface; (3) the thickness of the shell-wall; (4) the absence of sutures.

On turning to Dr. Rowe's specimen from the Margate Chalk, we notice the close resemblance of the shell-walls (Pl. VIII, Fig. 4*a*, *c*. and *r*.) with the Norwich example, the external surface in both being marked by strong radiating vertical costæ, crossed at regular intervals by well-marked transverse rings, forming with the costæ a delicate reticulated ornamentation like basket-work on the surface. In Dr. Rowe's specimen the opposite curved portions (*r*. and *c*.) appear

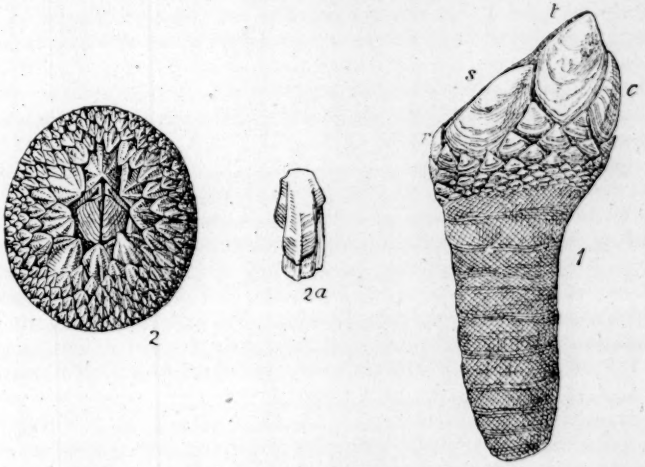



Fig. 1.—*Pollicipes polymerus*, G. B. Sowerby. Living: Upper California, Pacific, etc. (After C. Darwin's figure, op. cit., pl. vii, fig. 2.) "Capitulum with two, three, or more whorls of valves under the rostrum; latera regularly graduated in size from the uppermost to the lowest; scales of the peduncle arranged in close whorls." The range of the genus extends from the Rhætic beds; the Great Oolite, Stonesfield and Eyeford; the Oxford Clay, the Gault, Upper Greensand, Upper Chalk, the Eocene Tertiary, Isle of Wight; the Tertiary of Messina; and living in the seas of Europe, etc., at the present day.

Fig. 2.—*Catophragmus polymerus*, Darwin. Living: Australian Coast. (After C. Darwin's figure, op. cit., pl. xx, figs. 4*a*–4*e*.) "Interior compartments eight, with several exterior whorls of small supplemental compartments; basis membranous." "In large old specimens there are ten, or even more, whorls of compartments, but it is scarcely possible to count them with any accuracy." This genus does not occur in a fossil state.

Fig. 2*a*.—External view of one of the imbricated scales or valves, from the second whorl, counting from the inside.

at first sight to have been forced apart, or else that two additional lateral compartments of the shell-wall have fallen out and been lost; but this does not seem to have been the case. The important difference lies in the fact that, whereas in the Norwich specimen (Pl. VIII, Fig. 3) the shell-wall is exposed and bare to its basis, in the Margate specimen the base is concealed by a quite undisturbed semicircular quadruple row of shelly imbricated scales (Pl. VIII, Fig. 4*a*, *i.s.*, *i.s.*), analogous to those at the base of the capitulum of

Pedunculated Cirripedes (Lepadidæ), such as *Pollicipes mitella* (Pl. VIII, Figs. 2*a*, 2*b*) and *P. polymerus* (Woodcut, Fig. 1), but which are absent in ordinary sessile forms (Balanidæ).

Thus, in Dr. Rowe's specimen we have presented to us a Cirripede of the greatest interest, offering a most important connecting link between the more ancient PEDUNCULATA or LEPADIDÆ and the more modern OPERCULATA or BALANIDÆ.

Turning to the genus *Catophragmus* of Sowerby (Woodcut, Fig. 2), we find a sessile Balanid which assists us in the interpretation of Dr. Rowe's most interesting Chalk Cirripede, and also that Charles Darwin had, in 1854, already pointed out the significance of the structure of the shell in *Catophragmus* as a means of bridging over the interval between the sessile and pedunculated forms of Cirripedia which Dr. Rowe's specimen had suggested to my mind when he first placed it in my hands at the end of last year. "This genus of *Catophragmus*," writes Darwin,[1] "is very remarkable among sessile Cirripedes, from the eight normal compartments of the shell being surrounded by several whorls of supplemental compartments or scales: these are arranged symmetrically, and decrease in size, but increase in number towards the circumference and basal margin. A well-preserved specimen has a very elegant appearance, like certain compound flowers, which when half open are surrounded by imbricated and graduated scales. The Chthamalinæ, in the structure of the mouth and cirri, and to a certain extent in that of the shell, fill up the interval between the Balaninæ and Lepadidæ; and *Catophragmus* forms, in a very remarkable manner, the transitional link, for it is impossible not to be struck with the resemblance of its shell with the capitulum of *Pollicipes* (see Fig. 1). In *Pollicipes*, at least in certain species, the scuta and terga are articulated together; the carina, rostrum, and three pairs of latera, making altogether eight inner valves, are considerably larger than those in the outer whorls; the arrangement of the latter, their manner of growth, and union, all are as in *Catophragmus*. If we in imagination unite some of the characters found in the different species of *Pollicipes*, and then make the peduncle so short (and it sometimes is very short in *P. mitella*) that the valves of the capitulum should touch the surface of attachment, it would be impossible to point out a single external character by which the two genera in these two distinct families could be distinguished: but the more important differences in the arrangement and nature of the muscles, which are attached either to the opercular valves or surround the inside of the peduncle, would yet remain."

Although Dr. Rowe's Cretaceous Cirripede lacks the opercular valves, it enables us to conclude, from the presence of the three or four rows of imbricated scales around the base of the capitulum, that this form must at once be removed from the genus *Pyrgoma*, with which, as one of the Balaninæ, it has only a very remote affinity.

[1] A Monograph of the Subclass Cirripedia: The Balanidæ, etc., pp. 485–7, pl. xx, fig. 4. Ray Society, 1854.

Nor can we place it, as I at first conceived to be possible, in Darwin's subfamily Chthamalinæ, which embraces *Chthamalus*, *Chamæsipho*, *Pachylasma*, *Octomeris*, and *Catophragmus*, all of which are very irregular and aberrant forms of Balaninæ, of which the same author observes that they differ in many important respects from the Balaninæ proper and approach the Lepadidæ, as, for instance, in the supplemental whorls of imbricated scales or compartments in *Catophragmus*, etc.

We should, I think, rather regard this Cretaceous type as an ancient pedunculated Cirripede, which, judging from the form and thickness of its carina and rostrum, appears to be assuming a more sessile condition of growth, and by a later and further modification may have become completely so.

From the undisturbed triple or quadruple arrangement of imbricated scales enclosing the base it is quite certain that the carina (*c.*) and rostrum (*r.*) (Pl. VIII, Fig. 4*a*) could not have united to form a conical shell-wall like that in *Pyrgoma anglicum* (Pl. VIII, Fig. 5), as I originally supposed, nor do I think it could have had other lateral compartments between *r.* and *c.* to complete the shell-wall on the *Balanus* type of structure, the large size of the scales in the centre suggesting rather that they were the sub-latera, as in the capitulum of *Pollicipes*. It seems much more probable that the scuta and terga and perhaps a small and narrow latus took part, as in *Pollicipes*, in building up the capitulum, the basis of which was protected by a series of imbricated shelly plates. In point of fact we have here a *Pollicipes* which has abandoned its peduncle, and whilst still retaining the rows of imbricated scales at the base of its capitulum, has settled down into the preliminary stage of becoming a permanently sessile form.

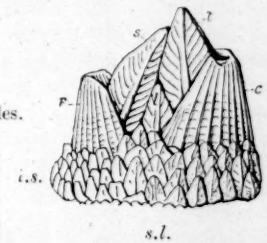



Fig. 3.—*Brachylepas cretacea*, gen. nov. (capitulum restored). The original figure of Dr. Rowe's specimen is here reproduced and restored by the addition of *l.* latus; *s.* scutum; *t.* tergum. The rostrum (*r.*) and carina (*c.*) and the imbricated scales (*i.s.*, *i.s.*) are copied from the original figure.

As *Lepas* was the name originally given by Linnæus to embrace both the pedunculated and sessile species, the designation *Brachylepas* may serve to express the present type, which embraces characters apparently common to both divisions of Cirripedia. The trivial name *cretacea* is of course retained.

The new form should, I think, be placed in a separate family, intermediate between the Pedunculata and Operculata, as—

## Family **BRACHYLEPADIDÆ.**

### BRACHYLEPAS, gen. nov., 1901.

*Non Pyrgoma* (as applied by H. Woodw., 1865, Brit. Assoc. Rep., p. 321).

Valves about 100 in number; latera of lower whorl numerous; lines of growth directed downwards; peduncle absent.

BRACHYLEPAS CRETACEA, H. Woodw. (Pl. VIII, Figs. 4*a*, *b*.)

Capitulum with three or four whorls of valves under the rostrum; apparently only three rows under the carina; sub-latera larger than the rest. The base on the side figured shows about fifty-four[1] shelly imbricated plates or scales forming eighteen vertical rows, arranged partly in three and partly in four rows; they are smaller, narrower, and more pointed under the rostrum (*r.*), and largest and broadest in the centre below the *latus* (see restoration, Fig. 3, *l.*), as we see is the case in *Pollicipes polymerus* (Woodcut, Fig. 1), where the latera are regularly graduated in size from the uppermost to the lowest of the series. The scales under the carina (*c.*) are larger than those beneath the rostrum (*r.*); but they are narrower and more pointed than those of the lateral series (which are reproduced enlarged on Pl. VIII, Fig. 4*b*). The scales have a strong median ridge with lateral divaricating lines, giving the free-edges a delicately plicated border. The median ridge is narrower and sharper in the scales beneath the rostrum, and broadest on the lateral scales.

The carina (*c.*) is marked by strong vertical ridges, which are crossed by numerous finer encircling bands, running parallel to the base, giving to both the carina and rostrum a delicate reticulated surface. The walls of both are thick, and so far as can be seen quite smooth on the inner surface. On the opposite aspect of the carina to that drawn, the base of the capitulum is seen to be nearly wholly exposed and bare, save for the presence of three of the shelly scales which remain *in sitû* adhering to the carina, the largest of which is 4 mm. in length. The semicircular wall of the carina measures about 17 mm. near its base around its outer face, and its height on the side not covered by the sheath of imbricated scales is $8\frac{1}{2}$ mm. The rostrum is considerably smaller than the carina; it measures 15 mm. around the outer surface near the base, and is 6 mm. in height.

The sheath of imbricated scales covers the base of the rostrum, on the side drawn in the Plate, 2 mm. deep, and extends also 2 mm. below the base of the rostrum, the whole series of scales being a little over 4 mm. deep.

Viewed from above, the body-cavity, enclosed in the convexities of the carina and rostrum, is seen to be oval, being 8 mm. long by 6 mm. broad. The walls of the capitulum are very steep, the carina, which is also much the highest, seeming almost to overhang at its summit.

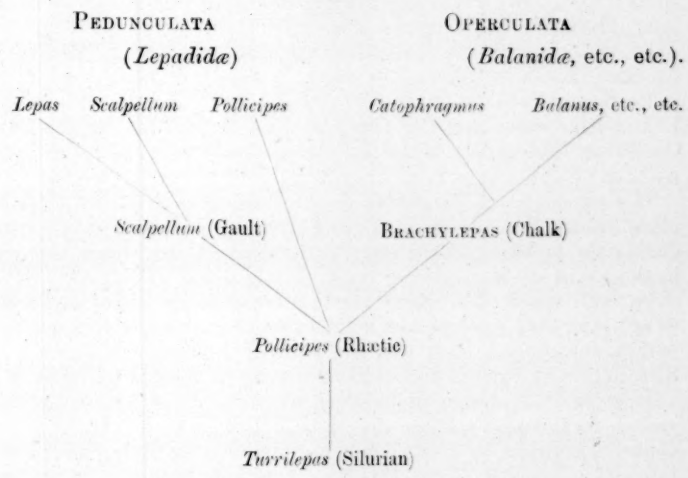
[1] That is, 54 plates on the side figured; if perfect, there would have been an equal number on the other side, or about 108 in all.

The imbricated scales or plates, which extend below the base of the rostrum and carina, spread outwards at a considerably wider angle than the capitulum. The attached valve of some small mollusc is seen adhering to the imbricated scales below the rostrum.

From the disparity in the proportions of the rostrum and carina, and the absence of alæ, we arrive at the conclusion that the terga and scuta were not mere opercular valves, but formed a part of the capitulum; that latera were also present is proved by the increase in size of the sub-lateral scales, which are much larger than those of the rostral or carinal series (see Pl. VIII, Figs. 4*a*, *b*).

There can, I think, be no reasonable doubt that *Brachylepas* forms a distinct family, from which at a later period probably the modern Operculata have arisen.

The place of *Brachylepas* in the phylogeny of the subclass may be indicated as follows:—




## EXPLANATION OF PLATE VIII, FIGS. 1–5.

FIG. 1.—*Balanus Hameri*, Asc. Recent: British. (Ad nat.) ½. (See also Darwin's "Balanidæ," p. 277, pl. vii, fig. 5.) *r.* rostrum; *c.* carina; *r.l.* rostro-lateral compartment; *l.* lateral compartment; *l.c.* carino-lateral compartment; *b.* basis; *ov.* opercular valves.

FIG. 2*a*.—*Pollicipes mitella*, Linn. Recent: East Indies. (Ad nat.) ½. *c.* carina; *t.* tergum; *s.* scutum; *r.* rostrum; *l.* latus; *s.r.* sub-rostrum; *s.c.* sub-carina; between *s.r.* and *s.c.* the valves of the lower latera are seen; *p.s.* peduncular scales.

FIG. 2*b*.—Four of the lower latera, with some of the peduncular scales enlarged. Three times natural size.

FIG. 3.—'*Pyrgoma cretacea*' = *Brachylepas cretacea* (the original specimen figured GEOL. MAG., Dec. I, Vol. V, 1868, p. 258, Pl. XIV, Figs. 1, 2). From the Chalk of Norwich. Preserved in the British Museum (Natural History). Enlarged twice natural size.

Fig. 4*a*.—*Brachylepas cretacea*. Specimen obtained and developed by Dr. Arthur Rowe, M.S., M.R.C.S., F.G.S., from the Chalk of Margate. Enlarged three times natural size. Original preserved in Dr. Rowe's cabinet. *c*. carina; *r*. rostrum; *i.s.*, *i.s.* imbricated scales at base of capitulum.

Fig. 4*b*.—Sub-lateral scales, enlarged six times natural size. From the centre of series just below the latus (see restoration in text, Fig. 3, *l*).

Fig. 5.—*Pyrgoma anglicum*, Leach (viewed from above). From the Coralline Crag, Ramsholt, Suffolk. Enlarged four times natural size. Recent: Great Britain, Europe, Cape de Verde. (Copied from Darwin's "Balanidæ": Pal. Soc. Mon., 1854, tab. ii, fig. 7*a*.)

## II.—Note on some Carboniferous Trilobites.

By Henry Woodward, LL.D., F.R.S., F.G.S.

(Plate VIII, Figs. 6–8.)

THE problems of life which the biologist is called upon to solve present so many and such varied aspects that they are never likely to become exhausted, or to weary by reason of their monotony. Among these the appearances and disappearances of groups in time (like the players on Shakespeare's mimic stage) are certainly not the least interesting questions awaiting solution.

In the case of the Trilobita, we are indebted to Walcott in America, Hicks in Wales, Lapworth in England, Peach and Horne in Scotland, Nathorst in Sweden, Mickwitz in Russia, and Holm in Lapland for extending the *Olenellus* zone back in time to the Lower Cambrian, thus giving to the Trilobites a vast increase in antiquity, without by any means reaching the dawn of life of this group.

The existence of Trilobites in the Carboniferous Limestone was made known as early as 1809, but no upward extension has occurred during the lapse of nearly one hundred years, save their discovery in the Culm of Waddon-Barton, Chudleigh, and Barnstaple, Devonshire,[1] still within the Lower Carboniferous series. One is tempted to ask, did they survive beyond the seas of the Lower Carboniferous period, and, if not, what was the cause of their extermination? To these inquiries our researches have at present yielded no reply.

It seems difficult to understand why the conditions which prevailed in the seas during the slow building up of the vegetable deposits of the Coal-period on the adjacent low-lying lands were inimical to the life of the Trilobita, seeing that near those old lands several species of small king-crabs (*Limuli*) were living, larger *Eurypterus*-like Crustaceans, small aquatic forms of *Cyclus*, numerous Brachyurans (the first lobsters), *Anthrapalæmon*, *Pygocephalus* (a Stomapod), with Phyllopod and Ostracod Crustaceans in great abundance: apparently offering an undoubted certificate as to the salubrity of this marine resort. Yet the Trilobites disappeared.

Although limited in the number of genera and species, the Carboniferous and Culm Trilobites form a most elegant and attractive group, but they do not display that great variety of form or ornamentation which characterized their predecessors in Silurian times.

[1] H. Woodward, "Trilobites from the Culm of Devon": Pal. Soc. Mon., 1884, Carboniferous Trilobites, pp. 59–70, pl. x. Also Quart. Journ. Geol. Soc., vol. li (1895), pp. 646–9.

Since I published my monograph on Carboniferous Trilobites (1883–1884, Pal. Soc. Mon., pp. 1–86, pls. i–x), I have given in this Magazine for 1894 (Dec. IV, Vol. I, pp. 481–489, Pl. XIV) descriptions of two new species, namely, *Phillipsia Van-der-Grachtii* and *P. Polleni*, from the Carbonaceous shale, banks of the River Hodder, Stonyhurst, Lancashire.

In November, 1895, I examined a number of specimens submitted to me by Dr. G. J. Hinde and Mr. Howard Fox, from the Culm of Devonshire and from a white siliceous rock at Hannaford Quarry, near Barnstaple. These represented forms already described as *Phillipsia Leei*, *Ph. minor*, *Ph. Cliffordi*, *Phillipsia*? (a larval form), *Griffithides acanthiceps*, *G. longispinus*, *Proetus* sp. A, *Proetus* sp. B (Q. J. G. S., vol. li, 1895, pp. 646–649, pl. xxviii, figs. 1–8). Mr. J. G. Hamling, Miss Partridge, and Mr. A. K. Coomara-Swamy, F.G.S., have also sent me specimens from Barnstaple for examination, some of which I hope to figure and notice shortly.

Last year, when visiting my friend Mr. E. Howarth, F.R.A.S., F.Z.S., the energetic Curator of the Public Museum, Weston Park, Sheffield, I discovered that this museum possesses a most excellent series of Trilobites from the Carboniferous Limestone of Derbyshire, of the existence of which I was previously unaware.

The collection was derived from two sources:—(1) Purchased with the geological collection of the Rev. Urban Smith, vicar of Stoney Middleboro' (near to Eyam), Derbyshire, an ardent geologist who during many years' residence in this district formed a large collection chiefly obtained from the Carboniferous Limestone of his own immediate neighbourhood. The specimen H. 88. 1103, *Griffithides longiceps*, Portlock, figured on our Pl. VIII, Fig. 6, enlarged three times nat. size, is from this collection. (2) The second collection was purchased as a part of the museum of Thomas Bateman, Esq., of Middleton Hall, near Bakewell, Derbyshire. Mr. Bateman wrote several books on the antiquities of Derbyshire and Yorkshire, and his archæological and geological collections were purchased for the Sheffield Museum (see Review of Mr. Howarth's Catalogue of Bateman Collection, GEOL. MAG., 1901, p. 37). The specimen H. 93. 118, of *G. longiceps*, figured on our Plate (Pl. VIII, Figs. 7, 8, enlarged three times nat. size), is from the Bateman Collection. From the Carboniferous Limestone of Wettin Hill, Derbyshire.

It is most rare to meet with specimens from the Carboniferous Limestone, such as the two here figured, in which the head, thorax, and abdomen (or pygidium) are preserved united in the same individual; the thoracic segments are very commonly absent, and the head-shield and pygidium are usually found separately, so that their description is often attended with considerable difficulty and uncertainty.

I set out with the full conviction that the above examples, the details of which are so remarkably well preserved, entitled them to specific distinction; but after more careful study I can only conclude them to represent a more slender variety of *G. longiceps*, the axis of

which is distinctly narrower than that figured by me in 1883 (Pal. Soc. Mon., pl. vi, figs. 7 and 8).

The description given at pp. 33–34 closely agrees with our present specimens, save in one particular, namely, the axis is there stated to be equal to half the entire breadth of the thorax, whereas in Fig. 6 of our Plate VIII it is shown to be exactly one-third the entire breadth of the thorax. This form might therefore be recognized as *Griffithides longiceps*, var. *angusta*, H. W.

The following is a list of the Trilobites from the Carboniferous Limestone in the Sheffield Museum, all from Derbyshire :—

*Phillipsia Derbiensis*, Martin, sp., 1809.
,, *gemmulifera*, Phillips, sp., 1836.
,, *Eichwaldi*, Fisher, sp., 1825.
*Griffithides globiceps*, Phillips, sp., 1836.
,, *Carringtonensis*, Etheridge MS. (H. W., 1884).
,, *longispinus*, Portlock, 1843.
,, *seminiferus* (a very good specimen in 'Rotten stone'), Phillips, sp., 1836.
,, *longiceps*, Portlock, 1843.
,, ,, var. *angusta*. (Pl. VIII, Figs. 6–8.)
*Brachymetopus Ouralicus*, De Verneuil, 1845 (a large series of very good detached head-shields and pygidia).
*Proetus*, sp. ind. (some small detached pygidia).

Some large detached pygidia in this collection may be new.

EXPLANATION OF PLATE VIII, FIGS. 6–8.

FIG. 6.—*Griffithides longiceps*, Portlock, var. *angusta*, H. W. Carboniferous Limestone: Stoney Middleboro'.

FIGS. 7, 8.—*G. longiceps*, var. *angusta*. Carboniferous Limestone: Wettin Hill, Derbyshire.

Figures enlarged three times natural size. Original specimens preserved in the Sheffield Museum.

## III.—NOTES ON THE GEOLOGY OF THE EASTERN DESERT OF EGYPT.

By T. BARRON, A.R.C.S., F.G.S., etc., and W. F. HUME, D.Sc., A.R.S.M., etc.

(By permission of the Under-Secretary of State for Public Works, and the Director-General of the Survey Department.)

The paper is divided into two parts, viz. :—

1. Sedimentary Rocks.
2. Igneous and Metamorphic Rocks.

PART I.—1. Pleistocene and Recent. (*a*) Igneous Gravel and Conglomerates.
(*b*) Newer and older Beach Deposits.
2. Pliocene. Nile Valley Limestones and Conglomerates.
3. Miocene Beds.
4. Eocene Limestones and Shales.
5. Cretaceous Limestones.
6. Nubian Shales and Sandstones.

1. (*a*) *Igneous Gravels, etc.*—These consist of granite, gneiss, and many other igneous and metamorphic rocks similar to those met with in the Red Sea Hills, and occur up Wadi Qena and spread

out in a fan-shaped delta at its mouth. Although abundant in this wadi, they are unknown in the side-valleys, even where they are now connected with the igneous hills. The explanation of this is as follows :—East of Qena the Eocene plateau has been broken up into a series of outliers, which until quite recently were connected by a long ridge, the sole break being that where Wadi Qena passes between the main plateau and the outlier of Abu Had. Previous to the formation of the latter fracture, the southern end of Wadi Qena was a bay, in which flint containing conglomerates and Pliocene limestones were being deposited, but when the above-mentioned gap was formed, the drainage from the Red Sea Hills passed through to the Nile Valley. Similar gravels cover the Red Sea Coast-plain. The age of these beds has now been shown to be Post-Pliocene, as there is a marked unconformity between them and the latter, and also because on the Coast-plain they are found underlying and overlying limestones containing Pleistocene fossils.

This throws a strong light on the age of the Nile. Mr. Beadnell found these gravels on the western side of the valley, and they are apparently continuous under the Nile alluvium, thus showing that the Nile as a river is later than these gravels, and could not have begun to flow until late Pleistocene times. These gravels are also suggested as the origin of the igneous pebbles reported by Professor Judd in the Royal Society's boring at Zaqaziq. All the rocks mentioned can be matched from the gravels near Qena. (Since this paper was read similar pebbles, but worn thin as by long rolling, have been found in cuttings in the lake deposits to the north of Heluan.) The theory of the derivation of the pebbles from the northern part of the Red Sea Hills is untenable, as it is known that Wadi Qena received all the drainage from that area in early Pliocene times.

These gravels are believed to have been deposited in a fresh-water lake, a series of which were formed as the sea retreated down the Nile Valley.

1. (*b*) ***Raised Beaches and Coral Reefs.***—Five series are recognized, of which the youngest is below sea-level, their succession being as follows :—

(1) The coral reefs at present forming in the Red Sea.
(2) The raised beaches and lower coral reefs which flank the coast, varying in height from near sea-level to 25 metres above the sea.
(3) A higher coral reef series on an average four to seven kilometres from the sea, and at various levels between 115 and 170 metres.
(4) A disturbed coral reef dipping 20 degrees eastward, closely related to the previous one.
(5) An old coral reef in which the affinities are as much Mediterranean as Erythræan, regarded at present as Miocene.

Along the shore 'storm-beaches' are common; in some places the shells form well-marked zones; while the higher beaches and reefs

are distinguished more or less from the lower by a different fauna. The disturbed reef has been formed previous to the formation of the parallel ranges of Jebel Esh and Zeit, thus bringing up the fault-movement to very recent times.

In this area there is an inversion of the stratigraphical arrangement, the higher beds being the older, the reefs being formed during a period of secular elevation. There is also apparently a long break between the two reefs, the explanation suggested being as follows:—

The first great Tertiary earth-movement in the Red Sea region was previous to the Upper Miocene and subsequent to the Eocene, the latter being faulted, and beds of the former deposited in the troughs produced. Later, as the result of further movements, coral reefs were formed on the sides of the igneous hills, but as soon as (owing to continued elevation and denudation) valleys had formed, down which torrents carried masses of pebbles, etc., the conditions became unfavourable for the formation of true reefs, and only gravels were deposited. This view assumes the existence of marked pluvial conditions, as maintained by previous writers, and it was only when the present desert conditions set in that the reefs again began to grow.

2. *Pliocene.*—Mayer-Eymar and Dawson have both regarded the Nile Valley as an arm of the sea in recent times as far up as Assuan. A foraminiferal limestone, found by Mr. Barron near Erment and which has been described by Mr. F. Chapman, contained two out of five species described not older than Miocene, while one is not known before Pliocene times, thus proving the above theory. Beds of the same age are found in Wadi Qena. They form a plateau consisting of flint conglomerates, white limestone, and at the base marls and fissile sandstones which vary greatly, the limestones being lenticular and thinning out to the east. The conglomerates are formed by the denudation of the Eocene limestone. The succession of these beds is as follows:—

(1) On the boundary-line with the Eocene rocks, breccias of flinty and cherty limestone with lenticles of limestone interbedded.
(2) Conglomerates of well-rounded pebbles.
(3) Pure white limestones, perhaps partly siliceous.
(4) Marls and clays.
(5) Sandy clays.

These beds are regarded as Pliocene on three grounds—(1) They have no resemblance to known Miocene beds in Egypt; (2) they are identical in all essential particulars with the foraminiferal series of the Nile Valley; and (3) the Pleistocene gravels are younger and unconformable to them.

These beds owe their origin to the faulting which produced the Nile Valley and Wadi Qena, and there must have been a subsidence of at least 400 metres to allow of their deposition.

The Pliocene has been a period of great movement marked by the formation of the great rifts such as the Red Sea, with the invasion of the fauna of the southern seas, the Gulf of Suez, the great scarp of the Red Sea Hills and its parallel ranges, and the main trend of

the Nile Valley and Wadi Qena, the two latter being in part arms of the sea extending far into the land.

3. *Miocene Beds.*—There are no new facts to be added to the results obtained by Mitchell and Mayer-Eymar in this area.

4. *Eocene Beds.*—These can be divided into two main series—(*a*) a thick group of limestones which are locally named Serrai Limestones, and (*b*) a thick group of shales, marls, and marly limestones termed by the Survey the 'Esna Shales.'

(*a*) The summit of the plateau is a bed containing a small nummulite, underlying which is a nodular limestone forming a distinct, precipitous, undercut cliff 3 metres high. Beneath this are limestones with flint-bands having a thickness of 200 metres, and having at their base a chalky limestone weathering pink. The total thickness of this series is 225 metres.

(*b*) The Esna shales are composed of yellow limestones (Pecten Marls) forming the base, succeeded by green shales, in the middle of which is a limestone, the total thickness being 122 metres. The Eocene here belongs to the 'Libysche Stufe' of Zittel or Londinian stage.

By the discovery of the unconformity between the Eocene and Cretaceous strata in Wadi Hammama, the presence of hitherto unsuspected Eocene has been proved on the eastern side of the Red Sea Hills, such as the faulted area of Jebel Duwi and Nakheil, near Qosseir, and the limestone range of Jebel Mellaha, near Jebel Zeit. The former is a bold white cliff facing south and dipping away at angles of 15 to 20 degrees, and is the result of complicated folding and strike- and dip-faulting, the flinty series being sometimes tilted at angles of 40 degrees, and lying in succession against Nubian Sandstone, metamorphic rocks, and granite, as in Jebel Hamrawein. Jebel Nakheil is an Eocene and Cretaceous syncline in which the succession is the same as that near Qena. Other outliers are noted in Wadi Hamrawein, the country north of Wadi Saga, at the confluence of Wadi Safaja and Wadi Wasif, and to the north-west of Wadi Um Tagher.

*Jebel Mellaha.*—Professor Zittel, in his map, following Schweinfurth's researches, refers the whole series to the Cretaceous, but the latter seems to have become aware of the presence of Eocene later. This range is composed of the same beds as Jebel Nakheil.

The Eocene beds have covered the whole of the Eastern Desert north of lat. 26° N., but have been entirely removed except where let down by faults. They are everywhere unconformable to the Cretaceous rocks.

5. *Cretaceous Limestones.*—After pointing out some gross errors recently published by Dr. M. Blanckenhorn, the most important points to be noted are these:—

(1) The occurrence of a Cretaceous plateau at Wadi Hammama containing numerous Cephalopoda, *Ptychoceras*, etc., and a coprolite bed about one metre thick, and extending over 20 kilometres to the north, where it runs to ground at the foot of Abu Had. The coprolite bed contains 50 per cent. phosphate of lime.

(2) There is a distinct unconformity between the Esna Shales and the Cretaceous.

(3) *Cretaceous Plateau at the foot of Jebel Duwi.*—This was hitherto scarcely known, and differs from the previously described area in the absence of the *Ptychoceras*, etc., and by their replacement by large Nautili, associated with *Libycoceras Ismaeli* and beds of *Trigonoarca multidentata*, etc., below which comes a bed crowded with *Ostrea Villei*. A strong unconformity is also here present between these beds and the Eocene. At the north end of this range, near Saga Plain, the coprolite beds are of unusual thickness.

(4) *Confluence of Wadi Safaja and Wadi Wasif.*—Here there are two well-developed coprolite beds, and a very prominent layer of *Baculites*. The conclusion arrived at is that the Cretaceous limestones of the area described are of shallower-water origin than those occurring to the north in Wadi Araba, and entirely Campanian in age, being characterized by the abundance of their oysters, their well-marked coprolite beds, and small thickness. This main type is of great palæontological variability, the beds near Qena, Qosseir, and Mellaha differing in essential particulars.

*Gypseous Deposits near the Red Sea.*—These occur only in the 'Raised Beach' area, and are almost always intimately connected with the limestones of this series. They crop out from under these beds, and, by their invariable unconformity and constant height above sea-level, suggest a "plain of marine denudation." They are the Lower Eocene and Cretaceous Limestones which have been altered, not from below as has been previously believed, but from above, as will be shown in the Report on Western Sinai by Mr. Barron.

6. *The Nubian Shales and Sandstone.*—These consist of soft green and black carbonaceous shales and marls, and dark-brown and red sandstone. The former being easily weathered are accountable for the formation of the large plains which are met with in the areas occupied by this series. The sandstones show evidence of ripple-marking, sun-cracks, rain-prints, and worm-tracks. In the softer upper beds, the vertebræ of a (?) *Mosasaurus* and pieces of fossil wood in excellent condition were found. It is everywhere unconformable to the underlying igneous rocks.

The age of the deposit in this district is Santonian or Lower Senonian, as shown by a bed of oysters found in the sandstone near El Geita by Mr. Barron. No traces of Carboniferous fauna have been discovered. It is later than the igneous range, and not earlier as maintained by Floyer and Mitchell.

## Part II.

Igneous and Metamorphic Rocks.—These rocks, forming a wide band running parallel to the Gulf of Suez and the Red Sea, practically constitute the mass of the Red Sea Hills. The latitude of 27 degrees N. closely agrees with an important geological boundary, the granites playing a considerable part among the components of the mountain ranges north of this line, while south

of it the metamorphic rocks become increasingly prevalent as the Qena-Qosseir road is approached, the granite forming sharp isolated ridges rising abruptly from among low hills of sheared diabase or slates. Almost on the southern edge of the area, well-marked gneisses and schists give rise to the range of Meeteg, whose rugged peaks dominate the upper portion of Wadi Sodmein.

Metamorphic Rocks.—In the following pages only the most important new facts can be touched upon, these being briefly as follows :—

*Gneiss, etc., near Qosseir.*—The northern track from Qena to Qosseir, after passing through a granite and dolerite region, suddenly enters a district composed of a grey, slightly schistose rock, breaking off into long splinters. Through it run numerous solution veins of quartz, bands of calcite and carbonate of iron, all of which have been extensively worked. These *slates, having a distinct satiny lustre*, and forming low ridges on the western edges of the two high ranges of El Rebshi and Meeteg, dip steeply south-west, but at the base of the former mountain system are replaced by underlying *green phyllites*, into which numerous dykes of dolerite have been intruded, quartz veins being also common.

The main range of Meeteg itself is composed of a still older series of quartz-mica schists, the younger members of which are of a yellowish colour, splitting readily into blocks more or less cubical in outline. Near the base of the mountain small veins of *granite penetrate into the schists*, in some places being pinched into these in a lenticular manner. The core of the range is composed of a *massive red and closely banded grey gneiss*, which, in a fine section displayed in the upper portion of Wadi Sodmein, is seen to be successively overlaid by a gabbro, mica-schists, *a massive dark dolerite*, hornblende-schists, and reddish-white mottled slates. From a little north of this point the valley wanders through a maze of hills of grey and green colour, consisting of micaceous, chloritic, and hornblendic 'schists,' capped by beds of dolerite and diabase, crushed or uncrushed. A question of terminology makes a difficulty, as the same term schist is here applied to these rocks in the foothills, which are far less compact than the typical varieties occurring in the main range.

*Sheared Diabases and Dolerites.*—The Wadi Sodmein section is useful because it shows the relative age of the gneiss and the sheared diabases, ashes, and other volcanic rocks, which occupy an enormous area of the southern portion of the Red Sea Hills, viz. 2,500 square kilometres approximately, being the main constituent of the region to the north-west and west of Qosseir, except where sedimentary hills have been faulted in. The sheared diabases and compacted ashes chiefly occur in this district, but further west, as in Wadi Atolla, are replaced by massive dolerites, which in many other localities are found in close association with volcanic members of many different types. This *volcanic* series is by no means limited to the area above mentioned, but reappears throughout the whole of the Red Sea region at most unexpected localities. Thus, in the

central range, dolerites and other basic rocks are seen capping some of the highest granite hills, still remaining as a thin coating, which otherwise has been almost entirely removed by denudation. Again, the base of the same range is fringed by a belt of the same character, the presence of which is probably directly referable to faulting on a large scale.

South-west of the central range, too, extends the Fatiri El Iswid district, consisting of range after range, in which dolerites, serpentines, compacted ashes, now practically slates, and agglomerates play an important part. While on the south of lat. 27° N. these rocks only give rise to low hills of complex character, to the north of that line they take part in the formation of scenic features of the first magnitude, rising to 1,800 metres in Jebel Dokhan, and composing some of the principal longitudinal ranges forming the eastern boundary of the Red Sea Hills.

The members of this volcanic series are of somewhat different character from those previously mentioned, dark andesites being as conspicuous as the dolerite sheets associated with them, while the sheared diabases have been replaced by tuffs and ashes far less compact than those near Qosseir. The agglomerates, too, are very striking in the El Urf chain, where blocks of 'imperial porphyry' are included among the rock fragments. Indeed, the most interesting member of this series is the imperial porphyry of Jebel Dokhan, typical specimens of which are withamite, containing andesites, though the same mineral is present in some of the tuffs.

*Relative Age of the Volcanic Series.*—It has already been stated that the dolerites, diabases, etc., rest upon the ***Metamorphic Schists and Gneisses,*** and are younger than the latter, but it is equally possible to show that the gneissose granites and diorites, which underlie them over wide areas, are of still later date. Thus, to take a few typical cases, a mass of mica-diorite has been intruded into the agglomerate, while in Wadi Esh, near Qosseir, the sides of the valley are formed of grey granite which is overlaid by the compact dolerite, but the former has sent numerous veins into the latter. Other examples will be mentioned in the report, but one of the best is close to the pass leading from Wadi Um Sidri to Um Messaid, where a dyke of red microgranite in the andesite has for a time prevented another vein of grey granite from penetrating into the lava, but finally, after running parallel for a short distance, the latter has succeeded in bursting through, and has sent long veins and branches into the porphyry.

*Granites.*—The rocks of granitic character in the Red Sea Hills are sharply divided into two groups, giving rise to very different types of scenery. The most prominent variety is a *coarse red granite,* poor in mica, which forms some of the finest summits north of and on the latitude of 27 degrees N., these being usually characterized by steepness, the mountains being seamed by bouldery ravines which cause the crests to have a highly serrated outline, while nearly all the lower country consists of bouldery ridges of a *gneissose* biotite, or hornblende granite, which has its south-eastern

boundary along a line joining Ras El Barud and Missikat El Qukh ranges. This gneissose granite is especially conspicuous owing to the abundance of the dykes of quartz-felsite and dolerite which vein it, in a north-east and south-west direction, the differential weathering of the two giving rise to a typical alternation of parallel ridges and sandy valleys to which the name 'dyke-country' may be applied. Where the above two varieties come in contact, it can be clearly seen that the red granite is the younger of the two.

### General Recapitulation.

We are now in a position to give the general succession for the Arabian Desert between Jebel Gharib and the Qena-Qosseir line.

1. The *metamorphic* are older than the igneous rocks.
2. The gneiss of Meeteg is the oldest member of the metamorphic series, the schists coming next in order, followed by slates, grauwacké (altered ash), sheared diabases, and dolerites.
3. Volcanic action had already begun during the period of formation of the grauwackés and slates, as the sheared diabases and dolerites are in places closely associated with them, but the main mass of the dolerite is younger than the slates. Thus the next in succession is a *volcanic* series in the south, consisting mainly of dolerites and sheared diabases, and in the north of dolerites, andesites, tuffs, and agglomerates.
4. These are themselves underlain, and in most cases intruded into, by a third series, a *quartz-diorite or grey granite*, in many cases gneissose.
5. Through the volcanics and grey granite rise masses of red granite, which may be almost contemporaneous with dykes of quartz-felsite and dolerite, seaming the members of the preceding series.

## IV.—Schists and Schistose Rocks in the Lepontine Alps: Reply to Criticisms by Professor A. Heim.

By Professor T. G. Bonney, D.Sc., LL.D., F.R.S.

SOME three years ago, on referring to the twenty-fifth volume of the "Beiträge zur Geologischen Karte der Schweiz," I found Professor Heim had devoted a few pages (pp. 316–319) of that work to my criticisms of his attempts to prove that Jurassic rocks had been metamorphosed into schists containing authigenous garnets, staurolites, etc. Had he brought forward any new fact of importance or pointed out any serious error in my work I should have replied at once, but as he was unable to do this, and as the justice of one of my criticisms was indirectly admitted in the petrographical appendix by Dr. C. Schmidt, I allowed more pressing and interesting matters to take precedence of one which had become chiefly personal.

On reading Professor Heim's remarks I perceive that we labour under a similar disadvantage, viz., that neither is a master of the language in which the other writes. He complains of a difficulty in understanding my meaning, though I think it was plain enough to most of my English friends. I am in the same position, because he appears to me to avoid the direct issues and to repeat assertions

which I have challenged. So, before going further, I will state the dispute as clearly and concisely as I can. It arose out of a paper read at the London Meeting of the International Geological Congress in 1888.[1] Then, or soon afterwards, Professor Heim made the following assertions: (1) that at Guttannen stems of a plant of Carboniferous age had been found in a gneiss; (2) that near Andermatt a crystalline marble was associated with a Jurassic limestone, so that they must be of the same geological age; (3) that in the Lepontine Alps a transition could be traced between fossiliferous Jurassic rocks and schists with authigenous garnets, staurolites, etc.

I have disputed the accuracy of all these statements. As regards (1) it is now admitted that the supposed stems are not organisms, but merely imitative markings. Hence this assertion is invalidated, but, as I have apparently made a mistake as to the nature of the rock, neither side in this controversy can 'score honours.'[2] About (2) there is nothing fresh to be said. I have discussed Prof. Heim's evidence, which he has not been able to strengthen, and think myself justified in claiming a verdict of 'not proven,' even if I have not shown his interpretation to be improbable.[3] My remarks accordingly will be confined to (3). Here sections are more numerous; the issue is simpler, and the initial difference between us largely concerns matters of fact. In the first place, Prof. Heim maintains that I have misunderstood him, and that he never affirmed those altered Mesozoic sedimentary rocks to be true crystalline schists. The very lax use of the term 'schist' by Continental and some English authors undoubtedly leads to confusion in expression as well as in thought, and I am prepared to admit that it might sometimes be difficult to draw a hard and fast line between a schistose rock (i.e. cleavage followed by a certain amount of secondary mineral development) and some foliated schists. This, however, does not really affect the present issue. Professor Heim asserted that certain schists with authigenous garnets, staurolites, etc., were proved to be of Jurassic age, not only by stratigraphical evidence, but also, where the minerals were less well developed, by the presence of fossils. I asserted that the schists with garnets, etc., were both truly crystalline and belonged to a group distinct from the Jurassic rocks in question; that this group could be shown to be much older than the Trias, and to differ in important respects from the fossiliferous schistose Jurassic rocks, which never contain authigenous garnets, etc., but only certain hydrous silicates, presenting a merely superficial resemblance to garnets, staurolites, etc. In other words, I gave reasons to show that Professor Heim's interpretation of the stratigraphical facts was untenable, and his identification of the important minerals was incorrect.

[1] Compte Rendu de la 4me Session, p. 80. See also *Nature*, Sept. 27 and Oct. 4, 1888, and Quart. Journ. Geol. Soc., vol. xlvi (1890), p. 236.

[2] Geol. Mag., 1900, p. 215.

[3] Quart. Journ. Geol. Soc., vol. xlvi (1890), p. 67; vol. l (1894), p. 285; vol. liii (1897), p. 16.

Passing now to the stratigraphy, I claim to have proved—

(*a*) That the schistose and fossiliferous Jurassic rocks in the Scopi and Nufenen districts overlie the rauchwacke.[1]

(*b*) That this rauchwacke (commonly a friable yellowish limestone, sometimes including layers of gypsum, but without any marked indications of metamorphism) often contains fragments of crystalline rocks corresponding with those which are elsewhere associated with the black garnet-bearing schists. Also, that this rauchwacke differs conspicuously from the crystalline limestone or dolomite, which occurs both on the northern side of the Campolungo Pass (south of the Val Bedretto) and in association with similar dark schists above Binn in the Binnenthal.

(*c*) That the rauchwacke generally overlies the group of crystalline schists, and where it is apparently interstratified with them a closer examination always suggests that it is a later rock 'nipped' in by overfolding and thrust faulting.

(*d*) In the noted Val Canaria section, where, according to Professor Heim, crystalline schists[2] are included in a fold of which an ordinary rauchwacke forms the base, not only does this rauchwacke contain fragments of more than one variety of the schists supposed to overlie it, but also the band of black garnet-bearing schists occurs *three* times, and the other beds are not in pairs. These facts prove a simple fold to be impossible,[3] and if faults be once admitted the key of Professor Heim's position is surrendered.

In addition to this I have shown, from stratigraphical, chemical, and microscopic evidence, that the schistose Jurassic rocks and this group of schists, locally garnet-bearing (a group which I have examined in many places from the Viso to the Gross Glockner, and to which I refer in my papers as the 'Upper Schists'), are quite distinct one from the other; the only possible confusion arising from specimens either badly preserved or in which their distinctive characters have been locally obliterated by extreme pressure. This, however, is no ground for asserting contemporaneity. In such rocks the metamorphism has been destructive, not constructive.

I pass, then, to the mineral differences. The group of schists, of which the dark one containing garnets is a member, consists, as I have shown elsewhere, of truly crystalline rocks, no less in the Val Canaria section than in the rest of the Alps, and never affords a trace of a fossil. Here and there in its dark schists are little streaks of crystalline calcite. These to a lively imagination may seem the ghosts of departed belemnites, but to a more prosaic mind they appear only a vein product. The rocks are true crystalline schists, no doubt of sedimentary origin, but greatly metamorphosed. They

[1] Quart. Journ. Geol. Soc., vol. xlvi (1890), p. 219, and vol. xlix (1893), p. 89.

[2] I suppose from what I have read that Professor Heim will refuse to call these rocks crystalline schists. If so, there is no crystalline schist—either garnet-mica, calc-mica, staurolite-mica, or quartz-mica schist—in any part of the Alps that I know of.

[3] The situation of the outcrops and their breadths make it impossible to escape this difficulty by supposing one black garnet schist to have been the top bed and to be doubled back on itself.

have been affected by pressure, but they were crystalline schists before that acted, for the larger minerals are sometimes distorted or even crushed, the garnets in one or two localities being distinctly cleaved. Pressure, in fact, has injured more than it has originated the crystalline condition. But the Jurassic rocks are only schistose; they have been affected by pressure, and it has produced the usual mineral changes on a comparatively small scale. But besides this, in some localities a number of ovoid and of rudely prismatic bodies have formed (the *knoten* and *prismen* of Von Fritsch). These, which occur along with fossils (belemnites, bits of crinoids, etc.), are not either garnets or staurolites, but only very impure silicates, more or less hydrous; some probably belong to the chloritoid, others perhaps to the scapolite group, with possibly a third mineral of the same general character.[1] Professor Heim asks almost in a tone of triumph[2] whether I have not seen "die Verquetschungen und die Veränderung (Marmorisirung) in der Structur der Belemniten . . . . die ganz mit der Umänderung des Muttergesteines parallel geht." Certainly I have: indeed have mentioned it (loc. cit., p. 219). But by this question he shows that he can have given very little time to the study of metamorphism. Otherwise he would have known that this 'marmorosis,' notwithstanding its fine name, proves but little, for calcite is one of those minerals which are always ready to crystallize, and particularly so when it is 'organic.' We constantly see this illustrated in rocks (especially Palæozoic) from English localities. There are no signs of pressure, yet fragments of fossils may be often found under the microscope to become partially or even wholly crystalline, to the obliteration of the original structure. I have also seen tests or spines of echinids from the Chalk break as if they were crystalline calcite, and a fractured stalactite showing the cleavage surface of large crystals.[3] Professor Heim, however, seeks to minimize my criticisms by intimating that I am a prejudiced witness, and have from the first shown signs of a distinct bias (*tendenz*). Of this I am convicted by my own confession, because I stated that, when I saw the specimens on which he rested his case, and which he exhibited at Burlington House in 1888, "'Still, I was not quite satisfied . . . . for it was very difficult to understand how such a fossil as a belemnite could have retained its characteristic form while

[1] Dr. Schmidt admits the presence of clintonite (which name is now applied by Dana to the group including the species of chloritoid), and assigns the knoten and prismen to zoisite. Both minerals are so full of impurities that it is very difficult to come to any conclusion, but neither reminds me of zoisites, nor is any close relationship suggested by the analyses (quoted on p. 233 of my paper); and after reconsideration of my specimens I see no reason to change what I wrote in 1890 (Quart. Journ. Geol. Soc., vol. xlvi, pp. 232–234). Dr. Schmidt's petrographical description will be found in Beiträge, vol. xxv, Anhang, pp. 41–65.

[2] Beiträge, ut supra, p. 317.

[3] Though I think that, as a rule, I can distinguish a marble belonging to a group of crystalline schists from an ordinary Palæozoic or later limestone, even if pressure modified, I put more reliance on any silicates which may be associated with the calcite, and do not feel quite happy unless I can trace the rock into some schist composed largely of these.

molecular changes of such importance were taking place in the matrix of the rock.' Er sieht hier eine Thatsache, an der er zweifelt, weil sie ihm unerklärich vorkommt!" Well, then, I will tell Professor Heim why I was not quite satisfied. In the first place, if it be a sign of bias to reason inductively from careful and numerous observations, and to rely on the conclusions thus obtained so far as to view with some suspicion any new phenomenon which distinctly contradicts them, I admit the charge, and that unblushingly, for I believe this to be the method of science. The latter is said by a good authority to be organized common-sense. If in every-day life a number of credible persons agreed in stating that something had occurred—say a man had done an action which they had witnessed—should we not be justified in cross-questioning rather severely anyone who suddenly appeared to swear an *alibi*? Now all my work, and it was considerable, undertaken with the sole desire of discovering the truth—work which had obliged me to discard almost everything I learnt when young—had led me to conclusions different from what Professor Heim was asserting.[1] Inasmuch, then, as his "Mechanismus," while greatly impressing me in some respects, had created suspicions of his trustworthiness as a guide in petrology, I submit that I was justified in thinking it possible he might have made a mistake. The 'Thatsache' was in reality little more than his assertion.

But he will say that I was shown the specimens. Yes; and if Professor Heim had seriously worked at petrology he would know that conclusions founded on hand-specimens are much less trustworthy than those arrived at by examination of rocks in the field or under the microscope. Speaking for myself, I refuse, when the matter is difficult, to express an opinion on a hand-specimen, but require to have a slice prepared for the microscope. Moreover, it appeared to me, when looking at his specimens, that the matrix of the two sets, those with belemnites and those with real garnets, was somewhat different. Professor Heim would no doubt set down this to 'bias,' but it is really the almost unconscious effect of that experience which most persons acquire by long work at a particular subject. It is very similar to the power which enables a specialist to make a diagnosis of something which a physician, who has worked along other lines, would not perceive.

But he quotes another phrase to convict me of bias. "It was very difficult to understand how such a fossil as a belemnite could have retained its characteristic form while molecular changes of such importance were taking place in the matrix of the rock." This remark is evidently not intelligible to Professor Heim, so I will endeavour to enlighten him. The results of contact-metamorphism, to which I have paid considerable attention, most nearly resemble the crystalline schists. In them, so far as my experience goes,

[1] I may add that the general *tendenz* to minimize the effect of 'dynamo-metamorphism,' of which he accuses me (p. 316), has the same foundation. That is an important factor in producing change, but its effect has been often greatly overestimated. After ten years' work I adhere to the position adopted in 1890 (loc. cit., p. 223), which since then I have so often expressed that I am weary of repeating it.

garnet, and still more staurolite, are not formed until the materials of the rock have undergone such great molecular changes as to obliterate all traces of a sedimentary origin and convert the rock into a fairly coarse crystalline aggregate of quartz, brown and white mica, andalusite, and other minerals.[1] Under such circumstances, I believe that any calcareous organism, if it did not disappear and supply its lime to some silicate, would become unrecognizable. Only in one case, that of the Bastogne rock, have I seen well-formed garnets in a matrix apparently not very greatly altered. These, however, are rather abnormal specimens, and, as it has been lately demonstrated, occur under very abnormal circumstances.[2] My bias, then, was due to experience, which showed me the antecedent improbability of what Professor Heim was asking me to believe.

There was yet one other reason for my scepticism. In 1883 I crossed the Gries Pass to the Tosa Falls, wishing to see an Alpine route of some historical interest, and with no definite geological aim, for I had but recently begun to make any special study of the 'upper schists.'[3] Here are some extracts from my diary. Going up the Eginenthal I observed occasionally loose blocks "of a dark slaty or schistose rock, with rounded spots and irregular long darkish crystals, which I took for a kind of 'knoten schiefer' and got a specimen."[4] Later on I write—"At the head of this [upland basin] there is evidently a great piece of well-bedded rock, not highly metamorphosed, apparently folded in the more crystalline rock. To this apparently the 'knoten schiefer' belongs, for it was all about here, some of it being rather more schistose than what I had seen below." Again, on reaching the top of the pass, I record the presence of dark mica schist with garnets, "looking more highly altered than that below." From the Tosa Falls I crossed to the Binnenthal and studied the crystalline schists in that district.[5] Thus I was aware that in the Lepontine Alps two rocks existed in which some superficial resemblances were associated with real and important differences. In other words, I knew that Nature had been laying traps, so that exceptional caution was needed.

I think, then, I may claim that my 'bias' was the result of knowing certain facts in petrology and Alpine geology better apparently than Professor Heim, and thus was more than justifiable. May I ask, in conclusion, that if he thinks he can refute any of the statements in this paper he will abstain from fighting under the shelter of an official publication. There I cannot reply to him; so the combat is one *Ubi tu pulsas, ego vapulo tantum.*

[1] Quart. Journ. Geol. Soc., vol. xliv (1888), p. 11.

[2] See Miss C. A. Raisin: Quart. Journ. Geol. Soc., vol. lvii (1901), p. 55. A museum specimen labelled Pyreneite (from that mountain range) appears to be another instance.

[3] See Quart. Journ. Geol. Soc., vol. xlv (1889), pp. 96–99.

[4] This is a transcript of my field notes, in which I do not pick my phrases. I probably should not now use the words 'knoten schiefer.' What I meant to express was that it seemed in about the same state of alteration as a chiastolite slate.

[5] A fortnight later I paid my first visit to the Val Piora.

## V.—Oscillations in the Sea-level. (Part I.)

By H. W. Pearson.

(PLATE IX.)

WHEN man first began the study of the earth's surface, he encountered at the very beginning, along the borders of the sea-coasts, on the lowland plains, and even on the hills, certain puzzling phenomena, difficult of explanation. These perplexing observations seemed to testify, by means of ancient raised beaches, fossil oyster and mussel shells, dessicated salt marshes, fragments of wrecks, and even by ancient anchors in the hills, that at some unknown time in the past the sea had "formerly been where the land now was."

Straton of Lampsacus and Eratosthenes (between 200 and 300 B.C.) explained these facts by supposing that the Mediterranean and the Euxine had once been dammed by barriers at the Pillars of Hercules and at Byzantium, and that by the breaking down of these barriers "much that was formerly covered by water had been left dry."

Strabo (54 B.C. to 24 A.D.), holding Straton and Eratosthenes to be in error, insisted that explanations of these facts must be found either in inundations caused by upheavals of the sea bottom, or in actual subsidence of these lands beneath the level of the waters and their subsequent upheaval, his preference being given to the first-named cause, as he deemed that the humidity of the bottom would render it more liable to shifting.

Here was raised, in the early morning of scientific investigation, the greatest problem of geology, or of geography, and such little progress has been made in the settlement of this question during the two thousand years that have since passed over our heads, that to-day if it is asked, are these evidences of former submergence and upheaval due to changes in the sea-level itself, or are they due to movements in the crust of the earth, no man can make certain reply.

That this uncertainty has real existence can be seen from the examples of opposing opinions herein quoted.

Celsius in 1730, in explanation of the apparent upheaval of the Baltic shores, affirmed a variable sea-level. Playfair in 1802 and Von Buch in 1807, adopting the second hypothesis of Strabo, affirmed movement in the earth's crust.

Sir J. A. Picton contended that the level of the sea had not changed, that it is the land alone which has altered its level (Proc. Liverpool Geol. Soc., vol. vi, p. 38). Sir Charles Lyell insisted "that the level of the ocean was invariable," and that the "Continents are inconstant in their level, as has been demonstrated by the most unequivocal proofs again and again, from the time of Strabo to our own time" ("Principles," 9th ed., Appleton, p. 518). Le Conte says, "we may look upon the sea-level as fixed" ("Elements," p. 138).

In opposition to these statements of Picton, Lyell, and Le Conte, James Geikie says, "the more recent raised beaches may be likely enough due to oscillations of the sea-level itself, and not necessarily to movements of the land" ("Pre-historic Europe," p. 525).

N. S. Shaler also says, that some of the apparent upheavals and depressions of the land may be due to absolute changes in the sea-level ("Geological Record," 1875, p. 178); and these men are supported in their rejection of the old theory of Strabo, which had been adopted by Playfair, Von Buch, and Lyell, by Edouard Suess, Dr. Schmick, and Trautschold, the latter claiming that "many of the phenomena of sedimentation and deposition attributed by geologists to a subsidence of the crust are, in fact, due to periodic *oscillations* or *upheavals* of the oceanic surface" (*Science*, vol. iii, p. 342).

These citations demonstrate that the matter of the permanency of the sea-level is to-day one of the unsettled questions of geology, and I believe it to be more fundamental in its nature than any other unsolved geological problem. It should be of interest, then, to learn why these conflicting opinions between our great geologists have existence. Why have the teachings of Playfair, Von Buch, and Lyell, adopted for three-fourths of a century, been in the last quarter of a century questioned from every direction?

Investigation allows us to answer this question. The old beliefs, in the absence of knowledge, were based on inference. The latest beliefs, rejecting inference, are based on observation, on an enormous accumulation of facts, that were entirely unknown to Playfair and the other disciples of Strabo, and these facts it is impossible to explain through the older theory.

For instance, Playfair's argument, on which the theory of an invariable sea-level rests, is as follows:—"In order to depress or elevate the absolute level of the sea by a given quantity, in any one place, we must depress or elevate it by the same quantity over the *whole surface of the earth* [my italics], whereas no such necessity exists with respect to the local elevation or depression of the land" ("Principles," 9th ed., p. 523).

Now the very foundation of this argument, a position unimpeachable in the time of Playfair and of Von Buch, is to-day absolutely untenable. The hypothesis of Adhemar, the knowledge that great masses of ice at one time existed in the Northern Hemisphere, and that submergence of the Northern, coexistent with emergence of the Southern Hemisphere, must have been the necessary consequence, as demonstrated mathematically by Dr. Croll, by Lord Kelvin, by Archdeacon Pratt, by Fisher, Heath, Woodward, and many others,—these arguments, I say, teach us that the contention of Playfair, Von Buch, and Lyell, valid perhaps in its day, is no longer to be accepted, and if the theory of a variable sea-level is to be rejected, reasons more solidly grounded must be accorded us.

It seems now impossible to reject the idea that upheaval of the sea surface in the north, and subsidence in the south, may be going on at one and the same time, and in addition to this the writer has shown how a local *upheaval* of the oceanic surface in one hemisphere may, nay must, be coexistent with a local *depression* of this surface at some point in the same hemisphere, provided the slightest variation of flow in the oceanic currents shall take place. (See *The American Geologist*, Sept. 1899, p. 192.)

To this point my discussion has been general, my object being merely to show the present uncertainties as to our knowledge relating to changes in the sea-level, and to call attention to the fallacies on which the arguments of Playfair and Von Buch were founded. I would now introduce a branch of the same subject not alluded to in the previous argument. It is this:—

It is admitted by all, that most of the lowlands of the Northern Hemisphere have at some time in the past been submerged to less or greater depth beneath the sea. The evidences of *great* submergences, such as those discussed by Chambers in his "Ancient Sea Margins," or by Prestwich in his "Traditions of the Flood," or as shown by McGee in his "The Lafayette Formation," will not now be considered. To these submergences we are as yet unable to assign a date. I would study, then, those minor relative changes in sea and land, both of depression and elevation, that have occurred since historic times, changes upon which a date and the approximate amount of movement can be fixed, with the object of determining whether these upheavals and submergences show any evidence of being *periodic* in their nature. We may attribute these changes either to movement in the earth or to movement in the sea, it is immaterial which; the only question is, have these oscillations shown regular cycles in their occurrence.

If some period could be discovered which governed these minor changes, it would seem that the law controlling this period might be found, and the establishment of law, if such existed, and the consequent elimination of chance, might enable us to determine with more certainty than at present whether the actual responsibility for these recent changes should be placed upon an unstable earth or upon a shifting sea.

This question of recent periodic oscillations in the sea-level was forced upon me by certain facts, impossible to explain otherwise, derived from many years' study of the raised beaches of the world; these facts, owing to the nature of this paper, I cannot now set forth, but they assured me in the strongest manner *that a regular cycle had existed at the time these raised beaches were formed*, and that its present existence was almost a certainty. I therefore commenced search for this periodical vibration of the oceanic surface in the records of history and tradition, in the ancient cities of the old world, in the registered changes in the coastlines of all countries, including the American coasts since the time of Columbus.

The data thus collected are almost unanimous in their testimony; they point unerringly to a vibration period in the sea-level of about 640 years, an interval of about 320 years existing between periods of high and of low water.

The data inform us as well that at periods of high-water the submergence increased in amount going north; they tell us that at previous periods of low-water the sea stood *lower than* at present; and finally, they assure us that, following the law of change which has guided these vibrations in the past, we must expect higher water in the north in the immediate future. This raised sea-level in the

north should culminate within 200 years, while the advance should be visible within a few decades.

The points in the curve illustrating the variation in level of the surface of the sea were sought for and found under a system of reasoning adopted after consideration of the results obtained from the investigation of the raised beaches before mentioned. This investigation furnished me certain testimony strongly opposed to all my prepossessions, yet, if I had interpreted the records correctly, I felt compelled to adopt as logical conclusions the following theories:—

1. Since the carving of these ancient terraces there had been no movement of the earth's crust, but these terraces lay in position exactly as originally traced.

2. The date of these beaches is unknown, but they certainly antedate the historical period. I must therefore conclude that since the dawn of history no upheaval or subsidence of the earth's crust can have occurred, and explanation of the observed recent submergence and emergence of lands must be sought for in vertical movements of the sea itself, rather than in upheavals or depressions of the crust.

3. I had reason to strongly suspect, in fact I regarded it as almost certain, that at the time of deposition of these terraces alternate rising and falling of the sea-level had occurred, that the traces of this movement were plainly discernible, that I had good cause to suspect the present existence of these same cycles of alternate ascent and descent in the sea-level, and that if these oscillations existed they should be uniform in direction of movement over one hemisphere.

Impressed, then, with the logic of the facts which had led up to these conclusions, facts which are set forth in other papers, I started on a new research, seeking for evidence of these suspected cycles, of the approximate dates of their maxima and minima, and of the amount in feet of their vertical vibration.

The apparent absurdity of entering upon such a labour as this is manifest. On all sides we see evidences of alleged upheavals or depressions of land: we know, for instance, that Scandinavia, Scotland, all of Northern Asia, Alaska, and Texas are now rising out of the sea; we are told that the coasts of New Jersey, Long Island, Cape Breton, and Greenland are now sinking beneath the sea. Here were undeniable facts directly opposed to each other and to my assumption that these movements must be universal in kind over either hemisphere.

These conflicting facts, which seemed to deny and refute these other facts mentioned, as obtained from the raised beaches, and to the accumulation of which I have devoted so many years' labour, seemed to assure me of failure from the first; but notwithstanding the discouraging outlook, search was undertaken for evidence of these periodic vibrations in the oceanic surface, no hope being entertained at that time, however, of finding explanation of those discordant motions, existing in the same hemisphere, to which attention has been called. My only hope was that these fluctuations might be found periodic in their nature.

At the beginning I had been led to suspect some physical connection between the periodic swing in the magnetic needle and these oscillations in the level of the sea; therefore, as the half-period in the motion of the agonic line is about 320 years, I commenced search for evidence of a period of universally higher water in the north, culminating about 320 years distant in the past, or about the year 1570.

As my investigations progressed I soon met an obstacle. I found that the study of the Temple of Jupiter Serapis by Babbage, Forbes, Lyell, and others, demonstrated that the high-water was receding in Italy in the years 1503 to 1511 (see "Physical Geography," by A. J. Jukes-Browne, p. 46), and consequently my culminating point of 1570 must be moved backward to some period probably anterior to 1500, and my assumption that the present low-water period was now at its central position also needed adjustment; we must have passed the low-water minimum.

I next sought proofs that the emergence of the Temple of Serapis was *coexistent* with a corresponding emergence of every part of the Mediterranean shore-line, and these proofs are in incontestable existence; many of them I submit herewith, hundreds of them for lack of space I withhold. George Maw discovered "evidence of upheaval, in a uniform rise of about 25 feet in distant parts of the Mediterranean, of an old coastline, *exactly corresponding* with the amount of emergence of the shell-bored columns of the Temple of Serapis," and this testimony of Maw (see Rep. Brit. Assoc. for Advancement of Science, 1870, p. 80) I have verified by a hundred items of evidence perhaps unknown to him.

Satisfied at length that the elevated sea-level was certainly uniform over the Mediterranean, I extended my investigations to the shores of England, France, Holland, and the Baltic, to the Americas, and to the shores of the Pacific, seeking as before for evidences of a raised sea-level, central about the year 1500.

England supplies a wealth of evidence. I found that Queen Elizabeth in 1562 was granting many descriptions of land in the bed of a creek or waterway 'swawed' or dried up, "by reason of the receding waters" ("History of Romney Marsh," Holloway, p. 141), at the same time, nearly, that Ferdinand and Isabella, for the same reason, were conveying land in Italy, that had likewise "dried up" (Brown, "Physical Geog.," p. 46).

Having thus collected much evidence that the sea-level was falling in the period subsequent to 1500, I next sought data as to its rise at some earlier date. Much evidence as to this movement was found. For instance, in 1427 we find Henry VI perplexed and disturbed "by the excessive rising of waters in divers parts of the realm," and urging that remedy should be "hastily provided" ("History of Romney Marsh," p. 130).

Testimony such as this, as to the epoch of Henry VI, combined with a great mass of similar evidence, like the progressive submergence during the same period of the Fens of England and the lowlands of Holland, led me to believe that the waters in 1427 were

rising, and as I knew they were falling in England, Italy, and France about 1500, my conclusion was that somewhere between these dates, say from 1450 to 1475, I must expect to find the culminating period of that particular epoch of northern submergence.

From this preliminary examination I was led to believe that a high-water period must certainly have existed over the greater portion of the European shores, culminating not far from the year 1450. I therefore entered upon a more extended search for data, not only as to this particular epoch of an elevated sea, but for those other and more ancient changes which I had been led to suspect as stated.

For many years I pursued this search, carefully collecting and indexing every notice as to change of sea-level encountered in my readings, regardless of date or direction of movement. The data thus accumulated seem to me conclusive; periodic vibrations in the ocean level are certain beyond question. The present cycle appears to have a period of about 640 years, while the evidence points to a period of about 500 years only at about the time of the Christian era.

A portion of the data which have been used in establishing this curve (see Diagram, next page) is submitted herewith. Hundreds, however, of the facts used as ordinates are omitted, that this paper may not be swollen to unreadable size.

When this material had been mapped out, it was found that 300 points or more were aggregated in a compact body, central about the year 1475, and that each of these points bore testimony to a period of high-water at some part of the earth's surface north of the Equator; another aggregation of points, less numerous and each one indicating a low-water period, was found bunched between the years 1100 and 1200, central about 1150 to 1175. I thus laid out all these accumulated facts each in its proper place and position, and at the end found a dense haze of dots central about the years 825 and 325 A.D. and 250 B.C., these clouds representing high-water periods, and similar swarms of dots, each representing proofs of low-water, central about the years 600 and 100 A.D., with occasional and *conflicting* points, scattered indiscriminately along the line.

At this point, then, to complete my curve it was but necessary to draw a sinuous line through these preponderating masses of dots; this curve was drawn, and the result is shown in the accompanying Diagram.

I now examined as to what weight these conflicting points might have towards weakening my confidence in the general accuracy of the curve. Much labour has been given to this subject; many of these dots were removed by investigation, others I attribute with good reason to erosion of shore-lines or to accretion to shore-lines, as is now going on all over the world, and finally I decided that not one of these conflicting points could be depended upon as making serious objections to the correctness of our curve. The information was too uncertain in its nature; it lacked that element of the positive, the known, which pervaded the great mass of evidence on which the curve had been based.

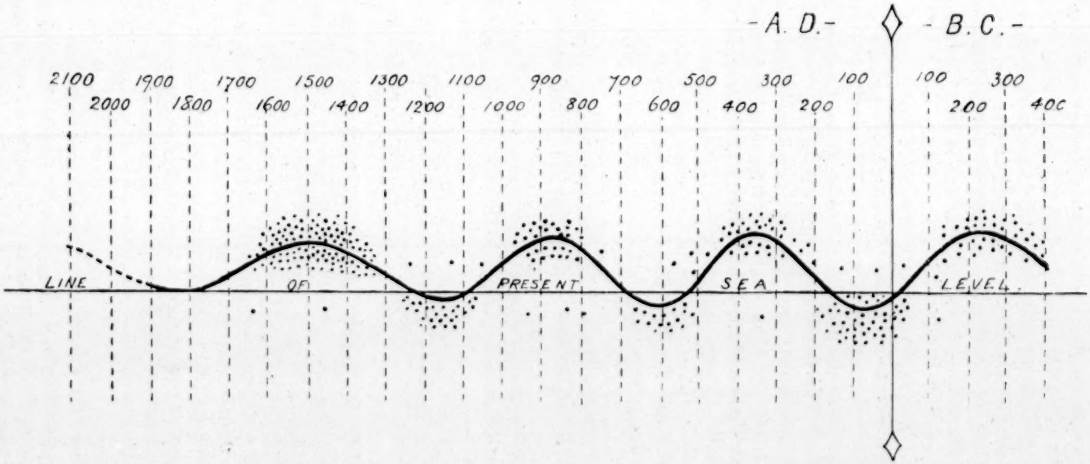



DIAGRAM to illustrate the periodic oscillations of sea-level in the Northern Hemisphere between B.C. 400 and A.D. 1900, deduced from a long series of historical records and observations by H. W. Pearson, Duluth, Minnesota, U.S.A.

For instance, Heligoland in the year 800 is shown in Myers' map to be of *great size*; this is in conflict with our curve, as the year 800 being near the high-water period, the island should have been *small* in size. On investigation we find that we have testimony equally strong that the island *was* small at that time. The description of the island by Adam of Bremen shows that it was *not much* larger than now in the time of Charlemagne (768 to 814), ("Principles," 9th ed., p. 329). (For this map of Heligoland in 800, see Von Hoff's "Geschichte," etc., vol. i, p. 56.)

Another item tending to invalidate our curve is the legend as to the submergence of lands now beneath the sea in Cardigan Bay, Wales. Pennant states that these lands—the Cantre'r Gwaelod—were overwhelmed by the sea about the year 500 (Pennant's "Tours in Wales," vol. ii, p. 274). In "The Gossiping Guide to Wales," however, we read, "We are not aware that any date is assigned" to this disaster (p. 37). It seems that what little is known of this inundation is derived from a poem by one Prince Gwyddno, who flourished between the years 460 and 520. There is no evidence that Gwyddno witnessed the event he describes, and it can be readily surmised that he merely reduced to verse the current traditions of an event that may have occurred three or four generations before his time. If this was the case, the Sarn Badrig and its attached legends would be evidence confirmatory of our curve. In any case we are assured that the date fixed by Pennant is uncertain and offers no reliable testimony against us.

(*To be continued in our next Number.*)

## NOTICES OF MEMOIRS.

ON THE STRUCTURE AND AFFINITIES OF FOSSIL PLANTS FROM THE PALÆOZOIC ROCKS. IV. THE SEED-LIKE FRUCTIFICATION OF *LEPIDOCARPON*, A GENUS OF LYCOPODIACEOUS CONES FROM THE CARBONIFEROUS FORMATION. By D. H. SCOTT, M.A., Ph.D., F.R.S., Hon. Keeper of the Jodrell Laboratory, Royal Gardens, Kew.

A SHORT account of the new genus *Lepidocarpon* has been given in a note communicated to the Royal Society last August;[1] the present paper contains a full, illustrated description of the fossils in question, together with a discussion of their morphology and affinities.

The strobilus of *Lepidocarpon Lomaxi*, the Coal-measure species, is, in its earlier condition, in all respects that of a *Lepidostrobus*, of the type of *L. Oldhamius*.

In each megasporangium, however, a single megaspore or embryo-sac alone came to perfection, filling almost the whole sporangial cavity, but accompanied by the remains of its abortive sister-cells. An integument ultimately grew up from the sporophyll, completely enclosing the megasporangium, and leaving only a narrow slit-like

[1] "Note on the Occurrence of a Seed-like Fructification in certain Palæozoic Lycopods": Roy. Soc. Proc., vol. lxvii, p. 306.

opening, or micropyle, along the top. As shown in specially favourable specimens, both of *Lepidocarpon Lomaxi* and of *L. Wildianum*, the more ancient Burntisland form, the functional megaspore became filled by a large-celled prothallus, resembling that of the recent *Isoëtes* or *Selaginella*. The whole body, consisting of the sporophyll, bearing the integumented megasporangium and its contents, became detached from the strobilus, and in this isolated condition is identical with the 'seed' described by Williamson under the name of *Cardiocarpon anomalum*, which, however, proves to be totally distinct from the Cordaitean seed so named by Carruthers.

The seed-like organs of *Lepidocarpon* are regarded by the author as presenting close analogies with true seeds, but as differing too widely from the seeds of any known Spermophyta to afford any proof of affinity. The case appears rather to be one of parallel or convergent development, and not to indicate any genetic connection between the Lycopods and the Gymnosperms, or other Phanerogams.

## REVIEWS.

E. Weinschenck. Zur Kenntniss der Graphitlagerstätten. III. Die Graphitlagerstätten der Insel Ceylon. Abh. k. bay. akad. Wiss., Cl. II, Bd. xxi, Abth. 11; München, 1900.

PROFESSOR Weinschenck has examined a series of rock and vein specimens from the graphite mines of Ragedara, Ampe, Pushena, and Humbuluwa, in Ceylon, collected by Dr. Grünling. He discusses the nature of the granulitic rocks and the mode of occurrence and origin of the graphite.

A general petrographical description of the granulitic rocks is given, illustrated by three plates of microphotographs. Massive habit, granulitic structure, and variable chemical composition are characteristic. Except in the more basic varieties, intergrowths of two felspars are very noticeable. The granulitic rocks include a continuous series ranging from aplites (weiss-steine) to pyroxene-plagioclase rocks (trapp-granuliten) and even pyroxenites. A rather oily lustre and greenish colour are very characteristic features. The constituent minerals are in a remarkably fresh condition, except in the immediate neighbourhood of the graphite veins. It is interesting to note that Professor Weinschenck does not mention any pleochroic monoclinic pyroxene.

There are certain other rocks in Ceylon which include coarse-grained dolomites and 'cipolins,' containing blue apatite and contact minerals such as forsterite, chondrodite, phlogopite, and spinel, and also the peculiar andalusite, sillimanite, and corundum bearing rocks described by Lacroix.

The granulitic rocks show no trace of the operation of dynamic causes; they are regarded as an eruptive mass which may form a single unit or be compound in character. The occurrence of coarse crystalline dolomites in the midst of the granulitic series seems to show that different eruptive units are separated by contact

rocks. The existence of still younger eruptive masses of granite has not yet been demonstrated, for the few rocks as yet described from Ceylon as granite are rather varieties of the granulitic series.

Professor Weinschenck compares the Saxon and Ceylon granulites, thinking with Naumann that the former are truly eruptive rocks. Had the Ceylon rocks been studied before those of Saxony this view would have been more widely held. They differ from the Saxon rocks chiefly in their non-schistose character and coarser grain. Lehmann regarded the peculiarities of the Saxon granulites as the result of dynamo-metamorphism. He regarded the microperthitic intergrowths of two felspars as the result of such a process, but as these are characteristic of quite unaltered rocks in Ceylon they may also be original in the Saxon rocks. The absence of sericite in the latter presents a difficulty to those who favour the dynamo-metamorphic view. Lehmann supposed that its place was taken by biotite, but this mineral is not infrequently an original constituent in Ceylon rocks. Garnets are characteristic of typical granulites, and their presence is the result of chemical peculiarities in the magma or peculiar physical conditions obtaining at the time of its consolidation. The chemical composition of Ceylon and Saxon granulites resembles those of truly igneous rocks. Perhaps in Saxony we are dealing only with the outer margin of an eruptive mass intruded into surrounding schistose rocks, while in Ceylon the heart of the eruptive mass is exposed. In both cases there has been extensive magmatic differentiation, and this may be considered characteristic of granulites in general.

It is only in immediate contact with the graphite veins that the granulite matrix is chemically altered and finally impregnated with graphite. Fragments of rocks included in the veins are also specially affected. In the altered rocks the felspars are largely changed to nontronite, a feature associated with the occurrence of graphite in the Passau district also. The pyroxenes change to a fine scaly material with aggregate polarization. Mica and garnet alter less readily. Impregnation with rutile and titanite is characteristic, as in the Bavario-Bohemian area. Beside the rock fragments, pieces of various minerals occur in the veins—quartz, pyrite, orthoclase, microperthite, apatite, biotite, augite—the formation of these being previous to that of the graphite, while calcite, and sometimes biotite, seem to have been deposited contemporaneously.

In the Passau district (Bavaria) the formation of nontronite and impregnation with graphite affect the whole schistose complex, while in Ceylon the graphite occurs in veins. This difference depends chiefly on the harder and more massive character of the Ceylon rocks. In Ceylon, Siberia, and Cumberland the graphite occurs in veins; in Passau and Taconderoga (U.S.A.) in veins and beds; in Bohemia in beds: these differences depend on the varied character of the matrix and not on different modes of origin of the graphite. Emanations of carbon monoxide, with or without cyanogen-bearing compounds, may have given rise to the graphite veins; while the introduction of iron oxide and manganese peroxide

in their neighbourhood may argue that metal carbonyls were also present.

Finally, Professor Weinschenck would suppose the following to have been the sequence of events in Ceylon:—A fluid magma intruded into beds of unknown age consolidated as a peculiar 'schlierig' rock, while contact-metamorphic structures were developed in surrounding beds. Contraction-joints developed on cooling, allowed the formation of pegmatites, including pure quartz veins to some extent. But, contemporaneously with the formation of the pegmatite, there were emanations of carbon monoxide and cyanogen-bearing compounds, which followed the same paths as the pegmatites and then gave rise to the graphite veins. The system of veins traversing the whole massif played in later mountain movements the rôle of buffer, the soft yielding mineral absorbing the mechanical effects, and thus the Ceylon granulites remained unaltered by dynamic changes.

I have attempted in this review merely to give an abstract of Professor Weinschenck's views as expressed in his important paper.

A. K. Coomara-Swamy.

## REPORTS AND PROCEEDINGS.

### Geological Society of London.

I.—February 15th, 1901.—J. J. H. Teall, Esq., M.A., V.P.R.S., President, in the Chair.

#### Annual General Meeting.

The reports of the Council and of the Library and Museum Committee for the year 1900, proofs of which had been previously distributed to the Fellows, were read. The Council stated that, although there was a decrease in the number of Fellows, the financial prosperity of the Society continued undiminished.

The report of the Library and Museum Committee enumerated the increasingly extensive additions made to the Society's Library.

The reports having been adopted, the President handed the Wollaston Medal, awarded to Professor Charles Barrois, F.M.G.S., of Lille, to Sir Archibald Geikie, for transmission to the recipient, addressing him as follows:—Sir Archibald Geikie,—

In these days of specialization few men are endowed with those faculties which enable them to contribute with marked ability to all branches of our many-sided science; but among those few Professor Barrois must unquestionably be ranked.

In the monograph on the Calcaire d'Erbray and many other papers he has established his reputation as a palæontologist: in numerous memoirs on the Granitic and Metamorphic Rocks of Brittany he figures as an accomplished petrologist; while in the many geological maps of the same district he has constructed a lasting monument to his skill and energy as a geological surveyor.

His published work represents a vast accumulation of facts carefully observed, clearly described, and lucidly arranged. More than this, it is often full of suggestiveness. He has had the satisfaction of initiating lines of research which have been followed up with great success by others.

It was he who first taught us how to zone our English Chalk by the aid of the fossils which it contains, and the friendships which he formed during the progress

of that work have been strengthened by the lapse of time. He might repeat with truth the words of another visitor to these Islands from the other side of the Channel: *veni, vidi, vici.*

In his recent publications on Brittany he has correlated the breadth and character of the metamorphic zones surrounding the granitic masses with the thickness of the cover under which the intrusions took place, and has suggested ideas that may prove of great importance in connection with such questions as the origin of the crystalline schists and igneous magmas.

But he has aided the progress of geology in other ways than as an original worker. The illustrious pupil of an illustrious master, he has contributed to maintain the great reputation of Lille as a centre of geological teaching; while his extensive knowledge and exceptional organizing ability have ever been at the disposal of the International Geological Congress and kindred associations.

Many years have elapsed since I had the privilege of making his acquaintance, and it is therefore with the greatest pleasure that I now ask you to transmit to him the Wollaston Medal, which has been awarded to him by the Council as a mark of their appreciation of the great services that he has rendered to all branches of Geological Science.

Sir Archibald Geikie replied in the following words:—Mr. President,—

It has been to my friend Professor Barrois a matter of very keen regret that he is prevented from being here to-day, to renew his personal relations with the Fellows of the Geological Society, and to receive from them the highest distinction which it is in their power to bestow. We must all deeply sympathize with him in the causes that deprive us of his presence. Bowed down by one of the greatest afflictions that can befall a father—the death of a son in the full bloom and promise of early manhood—he has manfully struggled with his numerous duties, until at last his health has given way under the strain. Let us hope that he may soon be restored to his former vigour, and be able to resume the researches in Brittany and the detailed description of them on which he has so long been engaged. He has asked me to receive this Medal for him, and I count it a great privilege and honour to be the intermediary between the Geological Society of London and one of the most distinguished and widely esteemed geologists of Europe. Professor Barrois has sent a letter of thanks, which I will now read:—

"Mr. President,—

"Allow me to express my gratitude for the new honour which the Geological Society has bestowed upon me, by the award of the Wollaston Medal, as I cannot but recall that the Council has on a former occasion encouraged me in my scientific work by the award of the Bigsby Medal.

"I have since made long wanderings along the Channel cliffs on both sides, from chalk to granite, for the sake of science, in the steps of De la Beche, Fitton, Godwin-Austen, and the founders of stratigraphical geology; and it is for me a very unexpected event to see my name written to day, for ever, with theirs, in the Proceedings of the Society.

"No distinction can be more gratifying to a geologist than to receive its highest award from the Council of the illustrious Society which for nearly a century has extended our knowledge in every branch of geology, and promoted progress in every part of the earth. I so greatly appreciate this great honour, that I feel as if the work that I have been able to accomplish was too small to merit the Wollaston Medal, granted as a reward, but rather as a friendly incitation to go on in my labour—'upward and onward.'"

Charles Barrois.

"Lille, February 9th, 1901."

The President then presented the Balance of the Proceeds of the Wollaston Donation Fund to Mr. Arthur Walton Rowe, M.B., M.S., of Margate, addressing him as follows:—Dr. Rowe,—

It will, I am sure, be a source of gratification to you to be associated with Professor Barrois on the present occasion, for you have done much to confirm and extend the principles which he first applied to the elucidation of the structure of the

English Chalk. We recognize, however, that, although your work has been of very great stratigraphical importance, your main object is biological, and that the task you have set yourself is that of working out the evolution of organic forms during the Upper Cretaceous period.

In your paper on *Micraster* you have set an example which I trust will be followed. You have shown how it is possible to deal with a vast mass of material, so as to bring out the main facts of evolution, without burdening science with hosts of new names and long lists of synonyms.

By the application of the dental engine to the preparation, and of micro-photography to the illustration, of fossils, you have also rendered signal service to science.

The Council of the Geological Society, in making this award, have been desirous of expressing their gratitude to you for the work that you have already accomplished, and their lively sense of favours to come.

In handing the Murchison Medal, awarded to Mr. Alfred John Jukes-Browne, B.A., of H.M. Geological Survey, to Mr. W. Whitaker, for transmission to the recipient, the President addressed him as follows:—Mr. Whitaker,—

Mr. Jukes-Browne, whose absence we all deeply regret, has aided the progress of geology in many ways. His numerous writings on the Upper Cretaceous Rocks are too well known to make it necessary for me to refer to them in detail. He has, from the first, recognized the enormous importance of associating palæontological with stratigraphical work, and by original research, as well as by a critical study of the writings of others, has made himself master of the geology of that period to which he has especially devoted himself.

But he possesses also a good all-round knowledge of geology. His Handbooks on Physical and Historical Geology have been of great service to students, and his suggestive work on the Building of the British Isles has been the means of directing attention to many problems of considerable theoretical interest.

There is yet another way in which he has rendered great service to geology, and that is as a stimulator of work in others. I am sure that no one will be more ready to acknowledge this than Mr. William Hill, with whom Mr. Jukes-Browne has been so long associated.

In recognition of these many services to our science, the Council have awarded to him the Murchison Medal, which I, an old College friend and fellow-student, now ask you to transmit to him with our heartiest good wishes.

Mr. Whitaker, having expressed his gratification at the privilege of receiving the Medal on behalf of an old colleague and valued friend, read the following extracts from a letter which he had received from Mr. Jukes-Browne:—

"I beg you to convey to the Council of the Geological Society my deep appreciation of the honour conferred upon me by the award of the Murchison Medal, and my great regret that the state of my health makes it impossible for me to be present in person to express my acknowledgments.

"That such work as I have been able to accomplish should be thought worthy of this high reward is not only a present gratification, but will be an incentive to show myself more worthy of such recognition. I feel also that I have been specially fortunate in my friends, and that without the assistance of two of them in particular —Mr. W. Hill and Professor J. B. Harrison—many of the investigations in which I have been concerned would have been incomplete.

"I should like further to say that the pleasure of receiving the Murchison Medal on the present occasion is much enhanced by the knowledge that the Wollaston Medal is at the same time awarded to my old friend Professor Barrois, whose zonal work among the Cretaceous rocks of England and France has added so much to our knowledge of those rocks."

The President then handed the Balance of the Proceeds of the Murchison Geological Fund, awarded to Mr. Thomas Sargeant Hall, M.A., of Melbourne, to Professor J. W. Judd, for transmission to the recipient, addressing him as follows:—Professor Judd,—

In awarding the Balance of the Proceeds of the Murchison Fund to Mr. Hall, the Council is desirous of recognizing the value of his many contributions to Australian Geology, and especially of his detailed researches on the Zonal Distribution of the Graptolites of Victoria. His work has thrown much light on the Lower Palæozoic history of Australia; while his discovery of the coincidence of the Ordovician auriferous belts with certain graptolitic zones is an illustration of the bearing of palæontological research on economic questions.

His application of the zonal method of research to the Kainozoic deposits of Victoria has done much to elucidate the later geological history of the colony, and his bibliographic labours have, I am told, greatly facilitated the work of his scientific colleagues in Victoria. We hope that this award will be of some assistance to him in further researches.

## In presenting the Lyell Medal to Dr. Ramsay Heatley Traquair, F.R.S., of Edinburgh, the President addressed him in the following words:—Dr. Traquair,—

The Council of the Geological Society, in presenting you with the Lyell Medal, desires to express its sense of the great value of your many contributions to Palæontology. More than thirty years have elapsed since the publication of your first papers on Fossil Fishes, and during the whole of that period you have been giving evidence of your keen insight into the structure of these interesting forms of life. I can only refer to one or two of your more important works.

Your memoirs on the structure of the Palæoniscidæ and Platysomidæ are, I believe, masterpieces of descriptive palæontology, and must for ever remain most valuable works of reference. Of great importance, from a geological point of view, have been your researches bearing on the fish fauna of the Old Red Sandstone of Scotland. You have not only shown the complete divergence between the fauna of the Orcadian Series and that of the Lower Old Red Sandstone south of the Grampians, but you have also pointed out that in certain areas the fishes in different divisions of that formation are arranged in life-zones—a fact which has been of service to the field-geologist.

Your last, and perhaps your greatest, work is your monograph on the remarkable Fossil Fishes from the Silurian rocks of the South of Scotland. Your keen insight and wide knowledge of fossil ichthyology enabled you to show, among other points, that the group of the Heterostraci, which hitherto contained only the Pteraspidæ, must be considerably enlarged, and that a transition could be seen from the shagreen-covered Cœlolepidæ to the plate-covered Pteraspidæ. You have also arrived at the conclusion that the Heterostraci, though not actual Selachians, had in all probability a common origin with the primitive Elasmobranchs. These results must be of the highest interest to biologists.

I have great pleasure in handing to you the Medal, together with our best wishes that you may long be spared to carry on your most valuable researches.

## Dr. Traquair replied as follows:—Mr. President,—

Permit me to thank the Council of the Geological Society for the honour which they have this day conferred upon me, and you, sir, for the kind words which you have spoken regarding my work.

I am much gratified to hear that some of that work has been of use to the stratigraphical geologist, as it is indeed impossible for the palæontologist who has himself collected in the field to avoid taking an interest in his subject from the geological standpoint also.

The impulse, however, which led me to take up Fossil Fishes as a speciality was entirely biological. While still a boy at school I broke open an ironstone nodule containing a piece of a Palæoniscid fish, and was thereupon seized by an intense curiosity to know how the bones of its head were arranged. As I did not find the information that I desired in the books, I resolved some day to try and work out the problem myself. Need I remark that, when in due time I got fairly to work on the subject, I found that fossil ichthyology presented a field sufficient to supply not only myself, but many others, with original work for our lifetimes?

If the work that I have accomplished in this field falls far short of the realization of early dreams, it is still gratifying for me to find that I have been able to do enough to merit this expression of the Society's approbation.

In presenting one half of the Balance of the Proceeds of the Lyell Geological Fund to John William Evans, D.Sc., LL.B., the President addressed him as follows :—Dr. Evans,—

Half the Balance of the Proceeds of the Lyell Fund has been awarded to you, in recognition of the importance of your geological work during the last ten years. Your visit to an almost unknown part of Brazil, and several years' residence in India, have enabled you to make observations and to collect specimens of great value to our science. The papers which you have already published in our Journal on the Matto Grosso district, and on the Calcareous Sandstones and Monchiquites of North-Western India, are evidence of your capacity for original work.

We trust that this award may aid you in publishing the results of investigations that you are known to have carried out while engaged in the Survey of the State of Junagarh (Kathiawar), and will encourage you in further work.

In handing the other half of the Balance of the Proceeds of the Lyell Geological Fund, awarded to Mr. Alexander McHenry, of the Geological Survey of Ireland, to Sir Archibald Geikie, for transmission to the recipient, the President addressed him as follows :—Sir Archibald Geikie,—

Mr. McHenry's claims to recognition are well known to you, and the fact that you receive the award of a moiety of the Balance of the Proceeds of the Lyell Geological Fund on his behalf is a proof that you cordially endorse the action of the Council. For forty years he has laboured to advance our knowledge of Irish Geology as a member of the Geological Survey, first as a collector of fossils and rock-specimens and afterwards as a member of the Surveying Staff. Most of his work has been published in the Maps and Memoirs of the Geological Survey, to which he has devoted himself, as you yourself have said, with admirable loyalty and enthusiasm. One of his most useful labours has been the preparation, in conjunction with his former colleague, Professor Watts, of a Guide to the Collection of Rocks and Fossils belonging to the Geological Survey of Ireland. His extensive and accurate knowledge largely contributed to make this work a most valuable compendium of Irish Geology. We hope that this award will act as an encouragement to him and be of some assistance in further work.

Sir Archibald Geikie, in reply, said :—Mr. President,—

On the part of my old colleague, I have to express to the Geological Society his best thanks for the recognition of his work which is expressed in this award. Next to myself he is the member of the Geological Survey who has been longest on the staff. His whole life has been devoted to his official duties, and he has only now and then ventured to make his appearance in non-official print. His labours are thus chronicled in the Maps, Sections, and Memoirs of the Geological Survey of Ireland, and are probably familiar to comparatively few geologists. He has been content honestly and strenuously to do his duty with a loyalty that has never flinched, and with an enthusiasm that seems to wax higher as the years go past. To such a man you may well believe that recognition from the Geological Society is as precious as it is unlooked for. It will nerve him with fresh energy for the task of revision of the Superficial Deposits of Ireland on which the Survey is about to enter; for it will show him that his work is not only known to his colleagues, but is appreciated by the leaders of Geological Science here.

In presenting the Bigsby Medal to Mr. George William Lamplugh, of H.M. Geological Survey, the President addressed him as follows :—Mr. Lamplugh,—

In 1891 the Council of the Geological Society recognized the value of your work on the Glacial Deposits of Yorkshire and on the Speeton Clay by an award from the Lyell Fund. Since that time you have still further extended our knowledge of the Lower Cretaceous rocks of Yorkshire and Lincolnshire, and have furnished Professor Pavlov with material which has enabled him to throw considerable light on the physical conditions and migrations of the Cephalopod fauna during the period represented by these rocks.

Your early work was done in the midst of an active and successful business career, which you gave up, somewhat against the advice of your friends, to join the Geological Survey and devote all your energy to the progress of science. Of late years you have been working in the Isle of Man, and the map of that island which you have produced is a striking proof of your skill as a geological surveyor. Its publication leads us to look forward with great expectations to the forthcoming memoir.

In awarding to you the Bigsby Medal, the Council feel that they are placing it in safe hands. You have done much, and they confidently expect that you will do more.

Mr. Lamplugh replied in the following words:—Mr. President,—

It is not without a proper sense of responsibility that I receive this Medal. The terms of the award leave no doubt that, while it is intended to some extent as a recognition of work already done, it is essentially intended as an incentive to further work, and implies a certain obligation in this respect, which you, sir, in your engaging words have not attempted to lighten. The recipients of this Medal in the past have always fulfilled the obligation, and it will indeed be a satisfaction to me if it be in my power to prove my fitness for the trust reposed in me by this award.

You have made reference to my altered circumstances since the time, ten years ago, when my earlier work received kindly recognition from the Council of this Society; and it may, therefore, be permitted me to confess that, in deciding to devote my whole energies to geological research, I felt some misgiving lest the studies which had proved so congenial as a recreation should take on another aspect when made the main occupation of my life. But the misgiving has proved groundless; the wider opportunity, so far from blunting my interest in these studies, has brought fresh zest, and on every side has opened up vistas of promising work for the future.

The President read his Anniversary Address, in which he first gave Obituary Notices of several Fellows and Foreign Members deceased since the last Annual Meeting, including Professor O. M. Torell (elected F.M. in 1883), Professor A. Milne-Edwards (F.M. in 1899); the Duke of Argyll (President in 1872–74); Mr. C. Tylden-Wright (elected a Fellow in 1857), Mr. G. C. Greenwell (el. in 1858), Mr. G. H. Morton (el. in 1858), General Pitt-Rivers (el. in 1867), Professor G. H. F. Ulrich (el. in 1867), Mr. J. Thomson (el. in 1868), Mr. C. J. A. Meÿer (el. in 1869), Mr. W. P. Sladen (el. in 1872), Dr. John Young (el. in 1874), and Dr. W. Waagen (el. in 1881).

He then dealt with the evolution of petrological ideas during the nineteenth century, especially as regards the igneous rocks. The discussions as to the origin of basalt and granite were referred to, and it was shown that the controversy regarding the latter rock had contributed largely to the clearing up of our ideas as to the nature of plutonic phenomena.

The solution theory propounded by Bunsen was especially emphasized, and its modern developments were briefly sketched. It was suggested that the next great advance will, in all probability, be the result of experiment, controlled by the modern theory of solutions, and carried out for the purpose of testing the consequences of that theory and discovering the modifications which may be necessary to adapt it to igneous magmas. The bearing which recent work on alloys had on petrographical problems was also referred to.

The problem of the origin of petrographical species was next considered, and the growth of ideas on the subject briefly sketched. It was pointed out that although magmatic differentiation is accepted by many as an important factor in producing different kinds of

igneous rocks, it does not rest on any assured experimental basis. Differentiation dependent on, or connected with, the crystallization of definite minerals was reviewed more favourably; but it was pointed out that all theories of differentiation which are based on unaided molecular flow are subject to the criticism that the time required to effect any important differentiation appears to be too great.

Reference was also made to recent work on the modification of igneous magmas by the inclusion and assimilation of rocks through which they pass; and the conclusion was reached that the origin of species, so far as igneous rocks are concerned, is a problem the final solution of which has been handed on by the nineteenth century to its successor.

The ballot for the Council and Officers was taken, and the following were declared duly elected for the ensuing year:—*Council*: W. T. Blanford, LL.D., F.R.S.; Sir John Evans, K.C.B., D.C.L., LL.D., F.R.S.; Professor E. J. Garwood, M.A.; Professor T. T. Groom, M.A., D.Sc.; Alfred Harker, Esq., M.A.; R. S. Herries, Esq., M.A.; William Hill, Esq.; W. H. Hudleston, Esq., M.A., F.R.S., F.L.S.; Prof. J. W. Judd, C.B., LL.D., F.R.S.; Lieut.-Gen. C. A. McMahon, F.R.S.; J. E. Marr, Esq., M.A., F.R.S.; Professor H. A. Miers, M.A., F.R.S.; Right Rev. John Mitchinson, D.D., D.C.L.; H. W. Monckton, Esq., F.L.S.; E. T. Newton, Esq., F.R.S.; G. T. Prior, Esq., M.A.; F. W. Rudler, Esq.; Professor H. G. Seeley, F.R.S., F.L.S.; Professor W. J. Sollas, M.A., D.Sc., LL.D., F.R.S.; J. J. H. Teall, Esq., M.A., F.R.S.; Professor W. W. Watts, M.A.; W. Whitaker, Esq., B.A., F.R.S.; H. B. Woodward, Esq., F.R.S.

*Officers*:—*President*: J. J. H. Teall, Esq., M.A., F.R.S. *Vice-Presidents*: J. E. Marr, Esq., M.A., F.R.S.; H. W. Monckton, Esq., F.L.S.; Professor H. G. Seeley, F.R.S., F.L.S.; W. Whitaker, Esq., B.A., F.R.S. *Secretaries*: R. S. Herries, Esq., M.A.; Professor W. W. Watts, M.A. *Foreign Secretary*: Sir John Evans, K.C.B., D.C.L., LL.D., F.R.S., F.L.S. *Treasurer*: W. T. Blanford, LL.D., F.R.S.

## II.—February 20, 1901.—J. J. H. Teall, Esq., M.A., V.P.R.S., President, in the Chair.

The Address, which it is proposed to submit to His Majesty the King, on behalf of the President, Council, and Fellows, was read as follows, and the terms thereof were approved:—

"TO THE KING'S MOST EXCELLENT MAJESTY.

"MAY IT PLEASE YOUR MAJESTY,

"We, your Majesty's most dutiful and loyal subjects, the President, Council, and Fellows of the Geological Society of London, humbly beg leave to offer to your Majesty our most profound and heartfelt sympathy in the great sorrow which has fallen on you in the death of our late beloved Sovereign Queen Victoria, and to most respectfully express the deep grief that we, in common with all your Majesty's subjects, feel at the great loss which has befallen the nation.

"While thus expressing our grief, we most humbly beg leave to offer to your Majesty our most sincere and unfeigned congratulations on your Majesty's accession to the throne of your ancestors. Our knowledge of the great interest which your Majesty has always taken in all matters relating to the welfare of your subjects makes us feel with confidence that science will continue to advance during your reign as in that of Her late Majesty of beloved memory. We recall with pride that your Majesty's father, the late Prince Consort, was for many years a Fellow of this Society.

"And we shall ever pray that your Majesty may long be spared to reign over a happy and contented people."

Professor J. B. Harrison, alluding to a series of views of parts of the interior of British Guiana, which he laid on the table, remarked that the photographs had been taken by his colleague, Mr. H. I. Perkins, F.G.S., Acting Commissioner of Mines in British Guiana, during their recent geological investigations into the structure of the goldfields of that colony. The views well illustrate the general characteristics of the densely wooded country in which the gold-bearing areas occur, and give some idea of the difficulties which affect the work of the mining prospector and of the field-geologist in that colony.

Several of the photographs illustrate rapids, cataracts, and falls which so frequently occur along the courses of some of the vast rivers of that part of South America, and show the differing forms of weathering of various igneous rocks and of horizontally-bedded sandstones and conglomerates in the tropics.

Among the photographs are several fine views of the Kaieteur Falls on the Potaro River, a tributary of the Essequibo. These falls, which were discovered by a Fellow of the Geological Society, Mr. C. Barrington Brown, in the course of his geological reconnaissance of the colony about thirty years ago, occur near the escarpment of the great sandstone formation which is so largely developed in the Guianas and in Brazil. The falls are over a ledge of very coarse siliceous conglomerate, some 18 or 20 feet thick, which overlies a thickness of about 1000 feet of almost horizontally-bedded sandstones. The river above the falls is about 400 feet broad and from 18 to 20 feet deep, and falls vertically, as a great curtain of water, for 740 feet, into a vast chasm at the extremity of a deep valley which it has eroded for a distance of about 17 miles from the escarpment of the sandstones. During the first 3 or 4 miles of its course from the falls through the valley, the river descends for about 400 feet by a series of cataracts and rapids. The valley, which is eroded in places through the sandstones into the underlying igneous rocks, is of surpassing beauty, and offers many features of marked geological interest. One of the views, taken when the water was low after a long-continued drought, shows very clearly the great cave which the spray of the falling water has cut out from the softer sandstone strata.

Others of the views show the somewhat primitive methods employed in prospecting and in working the placer-claims for gold.

Professor Edward Hull made a communication, illustrated by lantern-slides, on the submerged valley opposite the mouth of the River Congo. The position of this submerged valley has been ascertained by Mr. Edward Stallybrass and Professor Hull, by contouring the floor of the ocean with the aid of the soundings recorded on the Admiralty Charts. The sides of the valley are steep and precipitous and clearly defined, the width varying from 2 to 10 miles, and the length across the Continental platform being about 122 miles. It is continuous with the Valley of the Congo, and its slope is uninterruptedly downward in the direction of the abyssal floor. The steepness of the sides indicates that they are formed of very solid rocks.

Several other submerged valleys off the coast of Western Europe were described for comparison. In most cases the landward end of the submerged river-channel is filled with silt, etc., for some distance from the mouth of the actual river; but, farther out, its course becomes quite distinct towards its embouchure at the edge of the Continental platform. Among the valleys specified were those off the mouth of the Tagus and the Lima, the Adour, and the Loire, and those in the English and Irish Channels.

The following communication was read :—

"The Geological Succession of the Beds below the Millstone Grit Series of Pendle Hill and their equivalents in certain other parts of England." By Wheelton Hind, M.D., B.S., F.R.C.S., F.G.S., and J. Allen Howe, Esq., B.Sc., F.G.S.

Part i of this paper consists of a detailed account of the ground. Many detailed sections are given, showing in each case the exact fossiliferous horizons. The geological succession between the massif of limestone and the Millstone Grit Series on Pendle Hill is shown, by various sections, to contain a characteristic limestone series, easily distinguished by palæontological and lithological characters from the White or Clitheroe Limestone. This calcareous series is found to be very constant over a certain definite area, and to contain a zonal fauna.

By various sections the extent of the deposit is shown, and it is demonstrated that the deposit occupies a basin, of which the Pendle district covers the maximum area of deposit, for the sequence thins out rapidly north-wes and south. But although the beds thin out, a calcareous series with a typical zonal fauna is always present. Beds containing this fauna are traced from County Dublin, the Isle of Man, Bolland, Craven, the Calder and Mersey valleys, to Derbyshire and North Staffordshire. It is shown that this series, for which the term Pendleside Series is proposed, occupies a basin about the size of the area indicated above, and that the beds are lithologically distinct from the Yoredale Beds of Wensleydale, and contain a different fauna.

Part ii discusses the question in detail, from a palæontological point of view. Several goniatites and *Posidonomya Becheri* are shown to be characteristic of the lower part of the series, while *Aviculopecten papyraceus*, *Posidoniella lævis*, and certain goniatites have a wider distribution in the series.

The faunas of the Yoredale Beds of Wensleydale and the Pendleside Series, generally mapped as Yoredales, are shown to be entirely distinct; and the Yoredale Series of Wensleydale is shown, on palæontological and stratigraphical grounds, to be the equivalent of the upper part of the massif of limestone.

The migration of certain families of fossils from the north to the south, brought about by a slow change of environment, is shown by tables, and lines called 'isodiectic lines' are drawn to represent this distribution. It is shown that the Nuculidæ are found in the lowest Carboniferous beds in Scotland, but come in at successively higher horizons as the beds range southward.

These facts and comparative thicknesses are the basis of an argument as to the local distribution of land and water in Carboniferous times; and it is shown that the peculiar change in type which Carboniferous rocks undergo in passing from north to south is due entirely to physiographical conditions, and not to any theoretical assumption of contemporaneous faulting. It is shown, moreover, that the Craven Faults *per se* have had nothing to do with this change of type. The correlation of the limestone knolls of Craven with the Pendleside Limestone is demonstrated to be no longer tenable.

## CORRESPONDENCE.

### MR. A. R. HUNT ON THE AGE OF THE EARTH AND THE SODIUM OF THE SEA.

Sir,—In the volume of this Magazine for 1900 I reviewed Professor Joly's theory, that the age of the earth can be calculated by comparing the amount of sodium now in the sea with the time rate at which rivers are at present conveying sodium down. Among other matters I suggested that the salinity of rivers might be partly due to sodium derived from *sedimentary* rocks, which had formerly come from the sea. This would of course lengthen the computed age of the earth.

Mr. Hunt now suggests that "sea-water reached the heated rocks," and he appears to consider that much of the sodium, which the Dartmoor granites (at any rate) contain, was derived from the sea.

This is turning Professor Joly's theory round about. Professor Joly derives the salts of the sea from the igneous rocks. Mr. Hunt derives the salts of the igneous rocks from the sea.

My object in this letter is to direct attention to the difficulty of explaining the undoubted abundance of water, which is extravasated by volcanoes, to absorption from the ocean or from any other external source. I have gone into my objections to this view (whatever they may be worth) in my "Physics of the Earth's Crust" (2nd ed., p. 144), where I have, in a note, given an account of Daubrée's experiment, to which Mr. Hunt refers.

Since, alas! my two friends have passed away, it may be permissible to say, that I was on a visit to my dear friend Professor Prestwich shortly after he had published his paper on "The Agency of Water in Volcanic Eruptions," and Professor John Morris was my fellow-guest. We two were talking about Prestwich's theory that the volcanic water was derived *ab extra*, and that water could enter into combination with molten rock. Morris said, "Water would not be so foolish!" This was not a very scientific reason, but it was putting his own idea pretty strongly. He also told me that he had tried to dissuade Prestwich from publishing his views of volcanic energy, but without success.

My own opinion is that water has been a constituent of the liquid

interior of the earth from the very first, and that it simply makes its escape at a tremendous pressure whenever a way is opened for it through the solid crust. O. FISHER.

HARLTON.
*March* 5, 1901.

## NAMES FOR BRITISH ICE-SHEETS.

SIR,—May I suggest to Mr. Lamplugh that to propose names for British Ice-sheets before proving that they have existed is rather like counting chickens before they are hatched. At present we know neither the ancient extent of land-ice in our Island, nor in all cases what are indisputable traces of it. Where faith is strong this, no doubt, seems a detail, but to sceptics it appears important.

If, however, we admit that there was an East British Ice-sheet, "maintained and augmented principally by the snowfall upon its own surface," how are we to explain the presence of Scandinavian rocks at Cromer and other places on our East Coast? Of that ice-sheet the Dogger Bank would be centre and highest part. This tract is crossed (a little north of its centre) by a line drawn from Flamborough Head to the Naze of Norway. Over an area measuring about 70 miles from east to west, and 12 miles in the opposite direction, it rises above the ten-fathom contour-line (the minimum depth being 7 fathoms). The twenty-fathom line is very near to the other one at the south-west end, but then recedes from it so as to enclose a long bank which stretches in a north-easterly direction, almost half-way across the North Sea, and the thirty-fathom line on the northern side extends from the Yorkshire coast to Jutland. North and north-west of this limit are soundings down to 49 fathoms, and those over 40 fathoms are rather common. In the great channel off the south-west of Norway they are often over 200 fathoms (for particulars see this Magazine, 1899, p. 282). Thus the ice of the Dogger-fjeld (would not that have been a better name?) must have descended from its central plateau down slopes about 250 feet in vertical height on the north and north-west, and about half that amount down those from the south-west to the south-east. This mass of ice flowing outwards towards nearly all points of the compass, and buttressed on the western side by the Caledonian ice, which it would try to 'shoulder' in that direction, would surely defend our shores from the inroads of the Scandinavian ice-sheet, however nimbly it might climb the steep slope of the above-mentioned channel. Is it, then, a mistake to identify Scandinavian rocks in East Anglia; for if the Dogger-fjeld existed they could not have travelled on floating ice? T. G. BONNEY.

## CONCRETIONS OF CALCITE IN MAGNESIAN LIMESTONE.

SIR,—The well-known globular concretions from the Magnesian Limestone of Durham occur in many collections under the name of 'dolomite' or 'magnesian limestone.' Professor Garwood, however, effectually showed (GEOL. MAG., 1891, p. 436) that these concretions are due to the crystallization of calcite in a ground of

magnesian limestone, and that the 5 to 15 per cent. of magnesium carbonate contained in them is a mere impurity, when compared with the 30 per cent. in the matrix from which they have arisen. It is interesting to come across a similar statement made in 1817, though we waited long for Professor Garwood's numerical proofs, and for a complete account of the mode of origin of the concretions. Mr. N. J. Winch (Transactions of the Geological Society of London, vol. iv, p. 9) remarks that "botryoidal masses of fetid limestone devoid of magnesia, in balls varying from the size of a pea to two feet in diameter, imbedded in a soft, marly, magnesian limestone, are found at Hartlepool, etc." Winch had given a specimen some twelve years before to James Sowerby ("British Mineralogy," table 38), and the passage above quoted was incorporated by Conybeare & Phillips in their "Geology of England and Wales," 1822, p. 306. GRENVILLE A. J. COLE.

DUBLIN, *March* 1, 1901.

## SUCCESSION OF STRATA IN THE YOREDALE ROCKS.

SIR,—Mr. Dakyns is right in his criticism on the succession I quoted for the Yoredale strata of the Yore Valley. It is true that the sequence, though there are many exceptions, is usually—

Shale.
Limestone.
Sandstone.

But this may be put in another way. The series as a whole is made up of repetitions of this threefold cycle, and may with equal correctness be regarded as consisting of repetitions of the cycle—

Sandstone.
Shale.
Limestone.

We have, therefore, the same evidence of intermittent and more or less rhythmic sedimentation which I claimed for the Coal-measures. But there is this difference, that whereas in the Yoredales the cycle commences with inactivity (limestone) and proceeds to rapid sedimentation (sandstone), in the Coal-measures it commences with activity (sandstones and conglomerates) and proceeds to stagnation (coal-seams), the order being—

Coal.
Shale.
Sandstone and conglomerate.

Both formations result from rapid sedimentation over a subsiding area, but whereas the Coal-measures are essentially estuarine, the Yoredale rocks of the type developed in the Yore Valley bear every sign of having been laid down in open sea; the one was a product of the shallowest water, the other of comparatively deep water. Herein probably lies the explanation of the reversal of order of events.

I am obliged to Mr. Dakyns for the correction.

A. STRAHAN.

*March* 6, 1901.

## EVAPORATION AND SUBLIMATION.

SIR,—As long ago as September, 1900, I observe that the writer who reviewed my book on "The Scientific Study of Scenery" in this Magazine criticizes my use of the term *sublimation*.

He says: "In alluding to the evaporation of snow and camphor the process is referred to as 'sublimation.' In Watt's Dictionary of Chemistry sublimate is defined as 'a body obtained in the solid state by the cooling of its vapour.'"

Nevertheless, I believe that I use the term correctly, and in support of this assertion let me further quote Watt's Dictionary (1894 edition, vol. iv. p. 524). Sublimation is there defined as "The passage of a solid body, when heated, to the state of vapour without melting."

I take this opportunity of thanking the writer for the appreciative notice, which contains many suggestions which I should gladly utilize, if a second edition of my book should be called for.

J. E. MARR.

CAMBRIDGE.

## MALAY PENINSULA LIMESTONE.

SIR,—Since the publication of my paper in last month's GEOLOGICAL MAGAZINE,[1] where I compiled some notes on the geology of the Malay Peninsula, and took occasion to remark that in the absence of fossils it was impossible to correlate the limestones of that country with any definite horizon, some further samples of the same rock have been submitted to my notice by Dr. Henry Woodward, F.R.S.

This new material was collected a few years back by the late Mr. H. M. Becher, at Gua Sai, Penjom, Pahang, and is of precisely similar appearance to the paler-coloured limestones obtained by Mr. R. M. W. Swan from the River Tui District, which he found associated with those of a dark variety referred to in my paper.

The 'Becher' specimens are important from the fact that they exhibit organic structures, a feature pointed out by Dr. G. J. Hinde, F.R.S., on a manuscript label dated January 7th, 1899, who thus describes them:—"Very fine-grained bluish limestones. The only organisms recognizable are Crinoidal stem-joints. There are traces of other organisms with which the rock seems to have been filled originally, but they are now nearly obliterated and are not determinable."

This report, however, leaves us still without a clue as to the age of the limestone, and we shall require more accurate palæontological evidence before that desirable point can be permanently settled. In the meantime mention may be made of the presence of an obscure Crinoidal fragment on one of the weathered surfaces of this rock,

[1] "Notes on Literature bearing upon the Geology of the Malay Peninsula; with an account of a Neolithic Implement from that country": GEOL. MAG., 1901, pp. 128–134.

exhibiting a portion of the stem with fragmentary brachial extensions, the whole organism covering a space of nearly three inches in length. My colleague, Dr. F. A. Bather, has kindly examined the specimen, but without any satisfactory result, on account of its poor preservation; he is, however, inclined to regard it as of Palæozoic age. Further efforts should now be made to obtain more suitable fossils from these interesting limestones of the Malay Peninsula, so that their geological age may be finally determined.

R. Bullen Newton.

British Museum (Natural History).
*March* 19, 1901.

## OBITUARY.

### DR. GEORGE MERCER DAWSON,

C.M.G., LL.D., Assoc. R.S.M., F.R.S., F.G.S., F.R.S. Canada,
Director of the Geological Survey of Canada.

Born August 2, 1849. Died March 2, 1901.

This eminent geologist, whose portrait and life we published in the Geological Magazine for May, 1897, pp. 193–195, died at Ottawa, after an illness of only two days, at the early age of 51 years, sincerely regretted by a large circle of friends.

Dr. Dawson was the son of Sir William Dawson, F.R.S., for many years Principal of McGill College, Montreal; and was, since 1875, one of the staff of the Geological Survey of Canada, of which he speedily became Assistant-Director, and in 1894 Director. He was educated at McGill College, Montreal, and at the Royal School of Mines, London. Here he obtained the Duke of Cornwall's Scholarship, and the Edward Forbes medal and prize. He was, in 1873, on the North American Boundary Commission. On the Geological Survey he did much personal work in British Columbia and the North-West Territory, covering in his mapping many thousand miles of area. Dr. Dawson was one of the Commissioners for the Behring Sea Arbitration, spending the Summer of 1892 inquiring into the conditions and facts of seal-life, and his services were of the greatest value. He received the thanks of the Governor-General-in-Council, and was made a C.M.G. He received the Bigsby Gold Medal from the Geological Society in 1891, and in 1890 the degree of LL.D. from Queen's University and from McGill University in 1891. In 1897 he was awarded the Gold Medal of the Royal Geographical Society for his work as a whole.

Canada may well be proud of Dr. G. M. Dawson as one of her most brilliant men of science, whose loss she will long deplore, nor will he fail to be remembered in this country also as a son of that great Motherland whose name can never die.

## CHRISTIAN FREDERIK LÜTKEN.

BORN AT SORØ, OCTOBER 4, 1827. DIED AT COPENHAGEN, FEBRUARY 6, 1901.

PROFESSOR LÜTKEN, whose death, some two years after his resignation of the Directorship of the Zoological Museum at Copenhagen, removes another veteran from the ranks of the admirably trained and hard-working Scandinavian naturalists, was best known as a describer and classifier of living animals. But while, in common with the leaders of palæontology, he insisted that "only from the organization of the *living* form can we learn to understand that of the *extinct*," so also he was at one with the more eminent zoologists in recognizing that only by a study of extinct forms can we perceive the true relationships of the living. And it is because he put his creed into practice for over half a century that the close of his labours calls for the affectionate regret of geologists. That a notice should appear in this Magazine is moreover specially appropriate, since it was to it that he turned on the few occasions when he desired to address English readers in their own language. We allude to his notice of Lovén's memoir on *Leskia mirabilis* (GEOL. MAG., 1868, p. 179), his notes on the Ophiuridæ (1870, p. 79), and his criticism of Professor Kner's writings on the Ganoids and on *Xenacanthus* (1868, pp. 376 and 429). His own great memoir on the classification of the Ganoids appeared in *Palæontographica* (1873–75). From his many allusions to fossil Echinoderms we may select as early evidence of his penetration the constant opposition that he raised to the idea that the anus of the stalked echinoderms was a proboscis or mouth, and his severe criticism (oddly overlooked by later writers) of the division of the Crinoids into a Palæozoic and a Neozoic group. As a systematist the characteristics of his work were thoroughness, accuracy, and caution: qualities less showy than lasting. He was not a brilliant speculator on the phylogeny of unknown forms, but an advocate of, and an adept in, the synthetic method: "I mean that method which, giving up all preconceived ideas, patiently puts genus to genus, until families are formed, and family to family after their natural affinities, until the whole systematic building stands before us." It is work of this nature that will stand, that will vindicate the claims of palæontology to be heard, that will justify systematic zoology as a serious attempt to solve the problems of life, and that will keep science itself from the ridicule of the unlearned. We can ill spare such workers; but Lütken was a leader and a teacher as well as a student, and his monument is to be found not only in the books that he has left, nor even in the rich and well-arranged museum of Copenhagen, but also in the school of active and earnest zoologists that will long do honour to Denmark. F. A. B.

## ROBERT CRAIG.

WE regret to record the death at Glengarnock, on the 14th January, of Robert Craig, in the 80th year of his age. Mr. Craig took an active interest in geology, and from his occupation as

a quarrymaster and burner of lime he had exceptional opportunities for the pursuit of the science. During the past forty years he contributed many papers to the Transactions of the Glasgow Geological Society, more especially on the Drift deposits and Carboniferous rocks. In his own neighbourhood, from his literary and scientific tastes, he was known as "The Sage of Beith."

## MISCELLANEOUS.

Geological Survey of the United Kingdom.—We have already notified the appointment of Mr. J. J. H. Teall as *Director* in place of Sir Archibald Geikie, *Director-General*. The further appointments are two *Assistant-Directors*: Mr. H. B. Woodward (for England and Wales) and Mr. John Horne (for Scotland). *District Geologists*: Mr. C. Fox Strangways, Mr. Clement Reid, and Mr. Aubrey Strahan (for England and Wales); Mr. B. N. Peach and Mr. W. Gunn (for Scotland); and Mr. G. W. Lamplugh (for Ireland).

'Blood Rain' in Sicily.—A telegram despatched from Palermo yesterday stated that since the previous night a dense lurid cloud had hung over the town. The sky was of a sinister blood-red hue and a strong south wind was blowing, and the drops of rain which fell were like blood. The phenomenon, which is known locally by the name of 'blood rain,' is attributed to dust from the Sahara Desert having been carried there by the wind. Similar atmospheric conditions are reported from Rome. The sky had a yellow tint yesterday, and a violent sirocco swept over the city. At Naples showers of sand fell, and the phenomenon of the 'fata Morgana' was observed.—*Morning Post*, March 11, 1901.

Vienna, March 12.—Red and yellow snow has fallen in many parts of Austria, including districts so far north as Prague. The coloured snow lies several inches deep, and makes a weird and unearthly effect. Scientists state that southern winds of extraordinary force have carried the red and yellow sand of the Sahara across the Mediterranean to Southern Europe in such an enormous quantity that even here in Austria the colour of the snow has thereby been changed.—*Morning Leader*, March 13, 1901.

Reptilian Remains from Patagonia.—At the meeting of the Zoological Society on March 5th, Dr. A. Smith Woodward, F.L.S., F.Z.S., F.G.S., read a detailed description of the remains of *Miolania* from Patagonia, which were briefly noticed by Dr. Moreno in the Geological Magazine for September, 1899. He regarded them as indicating a Chelonian only specifically distinct from the typical *Miolania* of the Australian region. In the same formation in Patagonia were found the skeleton of a new extinct snake and the jaws of a large carnivorous Dinosaur, which were also described. The discovery of *Miolania* in South America seemed to favour the theory of a former Antarctic continent; but it should be remembered that in late Secondary and early Tertiary times the Pleurodiran Chelonia were almost cosmopolitan.